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OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

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VOL. III.—1874.

1816

TABLE OF CONTENTS.

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The Twentieth Ordinary General Meeting was held on Wednesday, the 14th January, 1874, Sir WILLIAM THOMSON, F.R.S., LL.D., President, in the Chair.

The President read his Inaugural Address as follows :—

GENTLEMEN,—I thank you most cordially for the great honour you have done me in electing me to be your President for the year 1874. Our first two Presidents, Mr. Siemens and Mr. Scudamore, in their interesting and valuable addresses, have explained the object of the Society of Telegraph Engineers, and have amply demonstrated its reason for existence. The success which it has already achieved, exceeding the most sanguine expectations of its well-wishers, must be very gratifying to its public-spirited founders, as a fruit earned by the toil and trouble they have voluntarily bestowed upon it. In numbers, in popularity, in usefulness, the Society of Telegraph Engineers has indeed grown with telegraphic speed.

When first addressed from the presidential chair, not quite two years ago, the Society consisted of 110 members. Since that time it has augmented to 500: including our Postmaster-General; the Directors-General of the great Telegraphic Administrations of Great Britain and India; many of the officers and operators of those systems and of the great Submarine Telegraph Companies; many scientific men interested in the subject, although not holding official positions in connection with practical telegraphy; and a list of distinguished names constituting our honorary and foreign members.

In his inaugural address our first president said, "Let us hope that our joint efforts may lead us in the direction of true scientific and practical advancement;" and we all know how strenuously and effectively he has himself laboured to promote the harmony of theory and practice, not only in the department to which this Society is devoted, but in all branches of the grand profession of engineering, of which he is so distinguished an ornament.

Before we commence the business of the session upon which we are now entering, may I be permitted to offer a few remarks on the relations between science and practice in engineering in general, but more particularly in telegraphic engineering. Engineering may be defined as the application of practical science to man's material circumstances and means of action. As usual in classification, the nomenclature of branches of engineering is full of what the logician calls cross-divisions. Thus we have civil and military engineering, and again, civil and mechanical engineering; then architecture and building, engineering and contracting. We have, it is true, in the distinction between military and civil engineering a good logical division. Every subject of civil engineering is included in military engineering, because an army has all the wants of any large body of civilians. But military engineering includes more, because there is no civil purpose which requires rifled cannon, shot and shell, congreve rockets, hand grenades, torpedoes, ironclads, armed fortifications, mining under fire or under liability to hand-to-hand encounter with an enemy, and field telegraphs. I have enumerated all the subjects which I can think of that belong exclusively to military engineering, and, except these, all subjects of general engineering are embraced in civil engineering, properly so called.

The division between military and civil engineering is, therefore, not properly founded on a distinction in respect of the subject matter, but it is a true logical division in respect to the province of application. Now remark the division between civil and mechanical engineering—a distinction habitually used, as if the engineering of merchant steamers, of cotton mills, of sugar machinery, of calico printing, of letter press printing, were not truly parts of civil engineering. I make no complaint of the ordinary language which designates as civil engineering only that which is neither military, nor concerned with mechanism otherwise than in designing and testing it, and which calls mechanical engineering the construction, daily use, and maintenance of machines. I make

no complaint of the ordinary language which so designates civil engineering, and distinguishes it from mechanical engineering. I only say that it is not logical. Take, again, architecture.

Architecture is not commonly called a branch of engineering at all. I think it unfortunate that the public do not regard architecture as a branch of engineering. When architects come to regard themselves as engineers, and when the public come to expect them to act as engineers, let us hope they will give us buildings not less beautiful and not less interestingly connected with monuments and traditions of beauty from bygone ages than they give us now. But assuredly there will then be less typhoid fever. Then invalids too ill to walk, or ride, or drive out of doors, or to be benefited by the beautiful scenery of Mentone, or Corsica, or Madeira, will not be expatriated merely to avoid the evil effects of the indoor atmosphere of England. Then people in good health will not be stupefied by a few hours of an evening at home in gas-light, or of a social reunion, or by one hour of a crowded popular lecture or meeting of a learned society. Then in our hotels, and dwelling-houses, and clubs, we shall escape the negatively refreshing influence of the all-pervading daily aërial telegraph, which prematurely transmits intelligence of distant and future dinners. The problem of giving us within doors any prescribed degree of temperature, with air as fresh and pure as the atmosphere outside the house can supply, may be not an easy problem; but it is certainly a problem to be solved when architecture becomes a branch of scientific engineering.

Now as to the relations between theory and practice in telegraphic engineering, I feel that I have more to say respecting the reflected benefits which electrical science gains from its practical applications in the electric telegraph than of the value of theory in directing, and aiding, and interesting the operators in every department of the work of the electric telegraph. In no other branch of engineering, indeed, is high science more intelligently appreciated and ably applied than in the manufacture and the use of telegraphic lines, whether over land or under sea; and it would be quite superfluous for me to speak on that subject to those whom I see before me.

But I do not know whether so much is thought of what the electric telegraph and its workers have already done, and may be expected yet to do, for science in general, and particularly electri-

city and magnetism. Time does not allow me to enlarge as I would like to do on this subject. I will merely remind those who are present of the great advance that has been made in accurate measurement within the last fifteen years. I need not tell you that a large part of the benefit thus achieved for science is due to the requirements of the practical telegraphist. Men of abstract science were satisfied to know that absolute measurement was possible, and that a definition of magnetic force, a definition of electric resistance, a definition of electromotive force, and so on through the list of numerical quantities in electricity, could each of them be stated in absolute measure.

We owe to Gauss and Weber the first great practical realization in abstract science of a system of absolute measurement; but their principles did not extend rapidly even in the domains of abstract science where their theory was well understood, because the urgent need for its practical application was not felt. When accurate measurement in any definite unit first became prevalent was when it was required by the electric telegraph. The pioneers of science in electric telegraphy, many of whom, happily for us, still work for science and for the electric telegraph, laid down—among various perfectly definite subjects for measurement—a unit of electric resistance—that most primary one of the different things to be measured respecting electricity. I need not remind any of you of the history of electric units of resistance, or of the labours of the Committee of the British Association to bring that system of measurement into harmony with the theoretical definitions of Gauss and Weber. The benefits conferred by introducing a system of definite measurement into the working of the electric telegraph are due not solely—perhaps not even in chief—to the application of Gauss's system, but to the introduction of very accurate and definite standards of resistance and means of reproducing those standards should the originals be lost. The benefit of putting the practical standards into relation with the science of Gauss and Weber has been set forth in the successive reports of the Committee of the British Association on electric measurement, and is well known, I believe, to most of the members of the Society of Telegraph Engineers.

But what I wish to say now is that theoretical science has gained great reflected benefit from the introduction of accurate measurement of resistance into practical telegraphy.

For many years measurements were performed in the office of

the telegraph factory, and at the station-house of the telegraphic wire, the means of doing which,—perhaps I might even say the principles on which those measurements were conducted,—being still unknown throughout the scientific laboratories of Europe. The professors of science who threw out the general principle have gained a rich harvest for the seed which they sowed. They have now got back from the practical telegrapher accurate standards of measurement, and ready means of transmitting those standards and of preserving them for years and years without change, which have proved of the most extreme value to the work of the scientific laboratory. I might make similar remarks regarding electric instruments. The theory of electric instruments has been taught by those who have laboured in theoretical science; but the zeal and ability with which the makers and users of instruments in the service of the electric telegraph have taken up the hints of science have given back to the scientific laboratory instruments of incalculable value.

But I wish rather to confine myself to looking forward to the benefits which science may derive from its practical applications in telegraph engineering, and to point out that this Society is designed by its founders to be a channel through which these benefits may flow back to science, and, on the other hand, to supply the counter-channels by which pure science may exercise its perennially beneficial influence on practice.

Time would fail me to give any such statement as would include a large part of the subject upon which I have touched; I shall therefore confine myself strictly to one point, and that is the science of terrestrial electricity. I have advisedly, not thoughtlessly, used the expression "terrestrial electricity." It is not an expression we are accustomed to. We are accustomed to "terrestrial magnetism;" we are accustomed to "atmospheric electricity." The electric telegraph forces us to combine our ideas with reference to terrestrial magnetism and atmospheric electricity. We must look upon the earth and the air as a whole—a globe of earth and air—and consider its electricity, whether at rest or in motion. Then, as to terrestrial magnetism,—of what its relation may be to perceptible electric manifestations we at present know nothing. You all know that the earth acts as a great magnet. Dr. Gilbert, of Colchester, made that clear nearly 300 years ago; but how the earth acts as a great magnet—how it is a magnet,—whether an electro-magnet in

virtue of currents revolving round under the upper surface, or whether it is a magnet like a mass of steel or loadstone, we do not know. This we do know, that it is a variable magnet, and that a first approximation to the variation consists in a statement of motion round the axis of figure—motion of the magnetic poles round the axis of figure, in a period of from 900 to 1000 years. The earth is not a uniformly magnetised magnet with two poles, and with circles of symmetry round those poles. But a first expression—as we should say in mathematical language the first "harmonic term"—in the full expression of terrestrial magnetism is an expression of a regular and symmetrical distribution such as I have indicated. Now, this is quite certain, that the axis of this first term, so to speak, or this first approximation, which, in fact, we might call the magnetic axis of the earth, does revolve round the axis of figure.

When the phenomena of terrestrial magnetism were first somewhat accurately observed about 300 years ago, the needle pointed here in England a little to the east of north; a few years later it pointed due north; then, until about the year 1820, it went to the west of north; and now it is coming back towards the north. The dip has experienced corresponding variations. The dip was first discovered by the instrument maker Robert Norman:—an illustration, I may mention in passing, of the benefits which abstract science derives from practical applications—one of the most important fundamental discoveries of magnetism brought back to theory by an instrument-maker who made mariner's compasses. Robert Norman, in balancing his compass-cards, noticed that after they were magnetized one end dipped, and he examined the phenomenon and supported a needle about the centre of gravity, magnetised it, and discovered the dip. Well, when the dip was first so discovered by Robert Norman it was less than it is now. The dip has gone on increasing, and is still increasing; but about fifty years ago the deviation from true north was greatest. Everything goes on as if the earth had a magnetic pole revolving at a distance of about twenty degrees round the true north pole. About 300 years ago it was on the farther side of the north pole, and a little to the east. Thence it travelled round the north pole in the same direction as that in which the earth itself rotates so as to come to the left-hand side of the north pole, and then obliquely towards us: so that about 200 years from now we may expect the magnetic pole to be between England and the north pole, and in England the

needle to point due north and the dip to be greater than it has been for one thousand years or will be again for another. That motion of the magnetic pole in a circle round the true north pole has already (within the period during which somewhat accurate measurements have been made) been experienced to the extent of rather more than a quarter of the whole revolution. It is one of the greatest mysteries of science—a mystery which I might almost say is to myself a subject of daily contemplation—what can be the cause of this magnetism in the interior of the earth? Rigid magnetization, like that of steel or the loadstone, has no quality in itself in virtue of which we can conceive it to migrate round in the magnetised bar. Electric currents afford the more favoured hypothesis; they are more mobile. If we can conceive electric currents at all, we may conceive them flitting about. But what sustains the electric currents? People sometimes say, heedlessly or ignorantly, that thermo-electricity does it. We have none of the elements of the problem of thermo-electricity in the state of underground temperature which could possibly explain, in accordance with any knowledge we have of thermo-electricity, how there could so be sustained currents round the earth. And if there were currents round the earth, regulated by some cause so as to give them a definite direction at one time, we are as far as ever from explaining how the channel of these currents could experience that great revolutionary variation which we know it does experience. Thus we have merely a mystery. It would be rash to suggest even an explanation. I may say that one explanation has been suggested. It was suggested by the great astronomer, Halley, that there is a nucleus in the interior of the earth, and that the mystery is explained simply by a magnet not rigidly connected with the upper crust of the earth, but revolving round an axis differing from the axis of rotation of the outer crust, and exhibiting a gradual precessional motion independent of the precessional motion of the outer rigid crust. I merely say that has been suggested. I do not ask you to judge of the probability: I would not ask myself to judge of the probability of it. No other explanation has been suggested.

But now, I say, we look with hopefulness to the practical telegraphist for data towards a solution of this grand problem. The terrestrial magnetism is subject, as a whole, to the grand circular variation which I have indicated. But, besides that, there are annual variations and diurnal variations. Every day the needle

varies from a few minutes on one side to a few minutes on the other side of its mean position, and at times there are much greater variations. What are called "magnetic storms" are of not very unfrequent occurrence. In a magnetic storm the needle will often fly twenty minutes, thirty minutes, a degree, or even as much as two or three degrees sometimes, from its proper position—if I may use that term—its proper position for the time; that is, the position which it might be expected to have at the time according to the statistics of previous observations. I speak of the needle in general. The ordinary observation of the horizontal needle shows these phenomena. So does observation on the dip of the needle. So does observation on the total intensity of the terrestrial magnetic force. The three elements, deflection, dip, and total intensity, all vary every day with the ordinary diurnal variation, and irregularly with the magnetic storm. The magnetic storm is always associated with a visible phenomenon, which we call, habitually, electrical; aurora borealis, and, no doubt, also the aurora of the southern polar regions.

We have the strongest possible reasons for believing that aurora consists of electric currents, like the electric phenomena presented by currents of electricity through what are called vacuum tubes, through the space occupied by vacuums of different qualities in the well-known vacuum tubes. Of course, the very expression "vacuums of different qualities" is a contradiction in terms. It implies that there are small quantities of matter of different kinds left in those nearest approaches to a perfect vacuum which we can make.

It is known to you all that aurora borealis is properly comparable with the phenomena presented by vacuum tubes. The appearance of the light, the variations which it presents, and the magnetic accompaniments, are all confirmatory of this view, so that we may accept it as one of the truths of science. Well now—and here is a point upon which, I think, the practical telegraphist not only can, but will, before long, give to abstract science data for judging—is the deflection of the needle a direct effect of the auroral current, or are the auroral current and the deflection of the needle common results of another cause? With reference to this point, I must speak of underground currents. There, again, I have named a household word to every one who has anything to do with the operation of working the electric telegraph, and not a very pleasing household word I must say. I am sure most prac-

tical telegraphers would rather never hear of earth currents again. Still we have got earth currents; let us make the best of them. They are always with us; let us see whether we cannot make something out of them since they have given us so much trouble.

Now, if we could have simultaneous observations of the underground currents, of the three magnetic elements, and of the aurora, we should have a mass of evidence from which, I believe, without fail, we ought to be able to conclude an answer more or less definite to the question I have put. Are we to look in the regions external to our atmosphere for the cause of the underground currents, or are we to look under the earth for some unknown cause affecting terrestrial magnetism, and giving rise to an induction of those currents? The direction of the effects, if we can only observe those directions, will help us most materially to judge as to what answer should be given. It is my desire to make a suggestion which may reach members of this society, and associates in distant parts of the world. I make it not merely to occupy a little time in an inaugural address, but with the most earnest desire and expectation that something may be done in the direction of my suggestion. I do not venture to say that something may come from my suggestion, because, perhaps, without any suggestion from me, the acute and intelligent operators whom our great submarine telegraph companies have spread far and wide over the earth are fully alive to the importance of such observations as I am now speaking of. I would just briefly say that the kind of observation which would be of value for the scientific problem is—to observe the indication of an electrometer at each end of a telegraph line at any time, whether during a magnetic storm or not, and at any time of the night or day. If the line be worked with a condenser at each end, this observation can be made without in the slightest degree influencing, and therefore without in the slightest degree disturbing, the practical work throughout the line. Put on an electrometer in direct connection with the line, connect the outside of the electrometer with a proper earth connection, and it may be observed quite irrespectively of the signalling; when the signalling is done, as it very frequently is at submarine lines, with a condenser at each end. The scientific observation will be disturbed undoubtedly, and considerably disturbed, by the sending of messages; but the disturbance is only transient, and in the very pause at the end of a word there will be a sufficiently near

approach to steadiness in the potential at the end of the wire connected with the electrometer to allow a careful observer to estimate with practical accuracy the indication that he would have were there no working of the line going on at the time. A magnetic storm of considerable intensity does not stop the working, does indeed scarcely interfere with the working, of a submarine line in many instances when a condenser is used at each end. Thus, observations, even when the line is working, may be made during magnetic storms, and again, during hours when the line is not working if there are any, and even the very busiest lines have occasional hours of rest. Perhaps, then, however, the operators have no time or zeal left, or, rather, I am sure they have always zeal, but I am not sure that there is always time left, and it may be impossible for them to bear the strain longer than their office hours require them. But when there is an operator, or a superintendent, or mechanic, or an extra operator who may have a little time on his hands, then, I say, any single observation or any series of observations that he can make on the electric potentials at one end of an insulated line will give valuable results.

When arrangements can be made for simultaneous observations of the potentials by an electrometer at the two ends of the line, the results will be still more valuable. And, lastly, I may just say that when an electrometer is not available, a galvanometer of very large resistance may be employed. This will not in the slightest degree interfere with the practical working any more than would an electrometer, nor will it be more difficult to get results of the scientific observations not overpoweringly disturbed by the practical working if a galvanometer is used than when an electrometer is available. The more resistance that can be put in between the cable and the earth in circuit with a galvanometer the better, and the sensibility of the galvanometer will still be found perhaps more than necessary. Then, instead of reducing it by a shunt, let steel magnets giving a more powerful direction to the needle be applied for adjusting it. The resistance in circuit with the galvanometer between cable-end and earth ought to be at least twenty times the cable's copper-resistance to make the galvanometer observations as valuable as those to be had by electrometer.

I should speak also of the subject of atmospheric electricity. The electric telegraph brings this phenomenon into connection with terrestrial magnetism, with earth currents, and, through them, with

aurora borealis, in a manner for which observations made before the time of the electric telegraph, or without the aid of the electric telegraph, had not given us any data whatever. Scientific observations on terrestrial magnetism, and on the aurora, and on atmospheric electricity, had shown a connection between the aurora and terrestrial magnetism in the shape of the disturbances that I have alluded to at a time of magnetic storm; but no connection between magnetic storms and atmospheric electricity, thunderstorms, or generally the state of the weather—what is commonly called meteorology—has yet been discovered. There is just one common link connecting these phenomena and those exhibited in the electric telegraph. A telegraphic line—an air line more particularly, but a submarine line also—shows us unusually great disturbances not only when there are aurora and variations of terrestrial magnetism, but when the atmospheric electricity is in a disturbed state. That it should be so electricians here present will readily understand. They will understand when they consider the change of electrification of the earth's surface which a lightning discharge necessarily produces.

I fear I might occupy too much of your time, or else I would just like to say a word or two upon atmospheric electricity, and to call your attention to the quantitative relations which questions in connection with this subject bear to those of ordinary earth currents and the phenomena of terrestrial magnetism. In fair weather the surface of the earth is always, in these countries at all events, found negatively electrified. Now the limitation to these countries that I have made suggests a point for the practical telegraphists all over the world. Let us know whether it is only in England, France, and Italy that in fine weather the earth's surface is negatively electrified. The only case of exception on record to this statement is Professor Piazzi Smyth's observations on the Peak of Teneriffe. There, during several months of perfectly fair weather, the surface of the mountain was, if the electric test applied was correct, positively electrified; but Professor Piazzi Smyth has, I believe, pointed out that the observations must not be relied upon. The instrument, as he himself found, was not satisfactory. The science of observing the atmospheric electricity was then so much in its infancy that, though he went prepared with the best instrument, and the only existing rules for using it, there was a fatal doubt as to whether the electricity was positive or negative after all. But the fact that there has been such a doubt is important.

Now I suppose there will be a telegraph to Teneriffe before long, and then I hope and trust some of the operators will find time to climb the Peak. I am sure that, even without an electric object, they will go up the Peak. Now they must go up the Peak with an electrometer in fine weather, and ascertain whether the surface of the earth is there positively or negatively electrified. If they find that on one fine day it is negatively electrified, the result will be valuable to science; and if on several days it is found to be all day and all night negatively electrified, then there will be a very great accession to our knowledge regarding atmospheric electricity.

When I say the surface of the earth is negatively electrified I make a statement which I believe was due originally to Peltier. The more common form of statement is that the air is positively electrified, but this form of statement is apt to be delusive. More than that, it is most delusive in many published treatises, both in books and encyclopædias, upon the subject. I have in my mind one encyclopædia in which, in the article "Air, electricity of," it is said that the electricity of the air is positive, and increases in rising from the ground. In the same encyclopædia, in the article "Electricity, atmospheric," it is stated that the surface of the earth is negatively electrified, and that the air in contact with the earth, and for some height above the earth, is, in general, negatively electrified. I do not say too much, then, when I say that the statement that the air is positively electrified has been at all events a subject for ambiguous and contradictory propositions; in fact, what we know by direct observation is, that the surface of the earth is negatively electrified, and positive electrification of the air is merely inferential. Suppose for a moment that there were no electricity whatever in the air—that the air was absolutely devoid of all electric manifestation, and that a charge of electricity were given to the whole earth. For this no great amount would be necessary. Such amounts as we deal with in our great submarine cables would, if given to the earth as a whole, produce a very considerable electrification of its whole surface. You all know the comparison between the electro-static capacity of one of the Atlantic cables, with the water round its gutta-percha for outer coating—and the earth with air and infinite space for its outer coating.* I do not remember the figures at this

* The earth's radius is about 630 million centimetres, and its electrostatic capacity is therefore 630 microfarads or about that of 1,600 miles of cable.

moment; in fact I do not remember which is the greater. Well now, if all space were non-conducting—and experiments on vacuum tubes seem rather to support the possibility of that being the correct view—if all space were non-conducting, our atmosphere being a non-conductor, and the rarer and rarer air above us being a non-conductor, and the so-called vacuous space, or the interplanetary space beyond that (which we cannot admit to be really vacuous), being a non-conductor also, then a charge could be given to the earth as a whole, if there were the other body to come and go away again, just as a charge could be given to a pith-ball electrified in the air of this room. Then, I say, all the phenomena brought to light by atmospheric electrometers, which we observe on a fine day, would be observed just as they are. The ordinary observation of atmospheric electricity would give just the result that we obtain from it. The result that we obtain every day of fair weather in ordinary observations on atmospheric electricity is precisely the same as if the earth were electrified negatively and the air had no electricity in it whatever.

I have asserted strongly that the lower regions of the air are negatively electrified. On what foundation is this assertion made? Simply by observation. It is a matter of fact; it is not a matter of speculation. I find that the air which is drawn into a room from the outside on a fine day is negatively electrified. I believe the same phenomena will be observed in this city as in the old buildings of the University of Glasgow, in the middle of a very densely-peopled and smoky part of Glasgow; and therefore I doubt not that when air is drawn into this room from the outside, and a water-dropping collector is placed in the centre of the room, or a few feet above the floor, and put in connection with a sufficiently delicate electrometer, it will indicate negative electrification. Take an electric machine; place a spirit-lamp on its prime conductor; turn the machine for a time; take an umbrella, and agitate the air with it till the whole is well mixed up; and keep turning the machine, with the spirit-lamp burning on its prime conductor. Then apply your electric test, and you will find the air positively electrified. Again—Let two rooms, with a door and passage between them, be used for the experiment. First shut the door and open the window in your observing room. Then, whatever electric operations you may have been performing, after a short time you find indications of negative electrification of the air. During all that

time, let us suppose that an electric machine has been turned in the neighbouring room, and a spirit-lamp burning on its prime conductor. Keep turning the electric machine in the neighbouring room, with the spirit-lamp as before. Make no other difference but this—shut the window and open the door. I am supposing that there is a fire in your experimenting room. When the window was open and the door closed, the fire drew its air from the window, and you got the air direct from without. Now shut the window and open the door into the next room, and gradually the electric manifestation changes. And here somebody may suggest that it is changed because of the opening of the door, and the inductive effect from the passage. But I anticipate that criticism by saying that my observation has told me that the change takes place gradually. For a time after the door is opened and the window closed, the electrification of the air in your experimental room continues negative, but it gradually becomes zero, and a little later becomes positive. It remains positive as long as you keep turning the electric machine in the other room and the door is open. If you stop turning the electric machine, then, after a considerable time, the manifestation changes once more to negative; or if you shut the door and open the window the manifestation changes more rapidly to negative. It is, then, proved beyond all doubt that the electricity which comes in at the windows of an ordinary room in town is ordinarily negative in fair weather. It is not always negative, however. I have found it positive on some days. In broken weather, rainy weather, and so on, it is sometimes positive and sometimes negative.

Now, hitherto there is no proof of positive electricity in the air at all in fine weather; but we have grounds for inferring that probably there is positive electricity in the upper regions of the air. To answer that question the direct manner is to go up in a balloon, but that takes us beyond telegraphic regions, and therefore I must stop. But I do say that superintendents and telegraphic operators in various stations might sometimes make observations; and I do hope that the companies will so arrange their work, and provide such means for their spending their spare time, that each telegraph station may be a sub-section of the Society of Telegraph Engineers, and may be able to have meetings, and make experiments, and put their forces together to endeavour to arrive at the truth. If telegraph operators would repeat such

experiments in various parts of the world, they would give us most valuable information. And we may hope that, besides definite information regarding atmospheric electricity, in which we are at present so very deficient, we shall also get towards that great mystery of nature—the explanation of terrestrial magnetism and its associated phenomena,—the grand circular variation of magnetism, the magnetic storms, and the aurora borealis.

And now, gentlemen, I must apologise to you for having trespassed so long upon your time. I have introduced a subject which, perhaps, more properly ought to have been brought forward as a communication at one of the ordinary meetings. I may just say, before sitting down, that I look forward with great hopefulness to the future of the Society of Telegraph Engineers. I look upon it as a Society for establishing harmony between theory and practice in electrical engineering—in electrical science generally. Of course, branches of engineering not purely electric are included, but the special subject of this Society is now, and I think must always be, electricity. Electric science hopes much from the observations of telegraphists, and particularly with the great means of observing that they have at their disposal. Science, I hope, will continue to confer benefits on the practical operator. Let our aim be to secure by organized co-operation that the best that science can do shall be done for the practical operator, and that the work and observations of practical operators shall be brought together, through the channels of the various sub-sections, into one grand stream which this Society will be the means of utilising.

Dr. Siemens (Past President)—Gentlemen, I have the pleasure of moving that the cordial thanks of the meeting be voted to the President, Sir William Thomson, for his able address this evening. I need not say that we have great cause for self-gratulation upon our having induced Sir William Thomson to assume the head of this Society. We shall increase our prosperity and our character as a scientific institution in no mean degree. Allow me, before I proceed further with my motion, to mention one or two points with regard to the condition of our Society at the present moment. You will presently be informed of the state of your finances, which, I am happy to say, are in the most prosperous condition. This prosperous condition is due to the great accession of members, and to the

liberal subscriptions to our publishing Fund which some members have given us; but it is due in a great measure also to the liberal spirit which animated the President and Council of the Institute of Civil Engineers in granting us this noble hall free of all expense. We are thus enabled to meet in great comfort, with all the prestige of the parent institution to support us. Some of us may have thought that we were rather in the way, perhaps, and might silently be considered what is called a "bore." If any such doubt existed in the minds of members it must have been completely dispelled in the case of those who attended last night's meeting here, when the President of the Institute of Civil Engineers mentioned the Society of Telegraph Engineers in the most flattering terms, and expressed his great pleasure at the Institution having lent a helping hand to this useful and prosperous Society.

We, however, are not quite dependant upon hospitality. We now have a home of our own—an office and a library. At No. 4, Broad Sanctuary, a very commodious office has been arranged, and shelves are now being provided for the reception of books. We hope to receive a good groundwork for a valuable library through the liberality of Sir Francis Ronalds, F.R.S., who before his death had promised to give it his favourable consideration, if not absolutely to commit to our charge his library, which is known to be a most valuable collection of works on electrical science. With such a foundation we hope to keep adding to our treasure, and, if members will only bear in mind that books on electrical and telegraphic subjects will always be most welcome, we shall from year to year have important additions made to our stock. On the other hand, members will have an opportunity of entering that library whenever they are in want of information on any subject regarding their profession.

We have heard to-night a most interesting address. It was an address on a subject upon which our President is pre-eminent above all others. It would be hard indeed to mention any branch of natural science in which he is not eminent, but on this subject of atmospheric electricity and electrometers, nearly all that has been done of late years is due almost entirely to the President of this Society. He throws out a suggestion which puts the Society at once in its proper light towards the advancement of science. Our President suggests that electrometer observations shall be made by telegraphic operators in all parts of the world with a view to

determine great cosmical and meteorological problems which occupy the minds of scientific men. This appears to me a most happy suggestion. Who could furnish such information but the telegraphic operator? And if through the Publishing Committee the information is collected from all parts of the globe, and put into our Journal from time to time, and tabulated there, I have no doubt that through this suggestion most important material will be furnished for the advancement of science.

Our President is not only a man of science of the highest eminence, but he also is a practical telegraph engineer, and as such we have a right to see him at the head of us, leading the Society towards the position which the founders of it hoped that it would soon attain—that of being both a scientific and practical Society, having chiefly in view the application of science to practical ends. I have great pleasure, therefore, in proposing that our cordial thanks be voted to our President, and that he be asked to allow his address to be printed in our Journal.

Professor Williamson, F.R.S.—I have great pleasure in rising to second the vote which has been proposed to you by my distinguished friend Dr. Siemens. It has been given to few to render such services to abstract science as those which have been rendered by Sir William Thomson. I may say with equal truth, even in this assembly, that it has not been given to many to render such services to electrical engineering as those which have been rendered by Sir William Thomson. But when we come to consider that one and the same man has attained such pre-eminent usefulness in the two opposite ends of human activity,—I mean the highest theory and the most thoroughly practical business,—then, I think, it must be felt that the Society has peculiar reason to congratulate itself on having so gifted a man at its head. I am sure that you must all have listened with much pleasure to the thoughtful and suggestive address which we have heard, and that you will most cordially vote the motion which has been proposed by my friend Dr. Siemens.

The motion was put by the mover, and carried with acclamation.

The President—Dr. Siemens, Professor Williamson, and Gentlemen, I thank you very much for the manner in which you have received my poor efforts in addressing you this evening, and for the kindness with which you have spoken of what I have been able to do for science.

The Meeting then adjourned.

The Twenty-first Ordinary General Meeting was held on Wednesday evening, the 28th January, 1874, Sir WILLIAM THOMSON, F.R.S., President, in the Chair.

The PRESIDENT announced that Mr. Louis J. Crossley, a Member, had presented the Society with eighty-two volumes of the Transactions of the Royal Society. The gift of eighty-two volumes of Transactions of the Royal Society, dating from the commencement, is one of great importance indeed, and they cannot fail to be of continual use to the members of the Society. These volumes extend nearly up to the present date, and a Member of the Council has kindly offered to put into the Library, on trust, the volumes completing the series up to the present date, so that members will thus have the valuable privilege of being able to consult in their own library any volume of the Transactions up to the present time. I have just signed a letter, in the name of the Council, thanking Mr. Crossley for this gift.

The PRESIDENT: The first subject before us this evening is the adjourned discussion on Mr. G. E. Preece's paper on Underground Telegraphs.

MAJOR WEBBER, R.E.: At the last meeting I ventured to offer a few remarks on the subject of Underground Telegraphs, and it occurred to me that probably this discussion would be continued by gentlemen who would from their experience be able to give us some results as to the progress and as to the past maintenance of underground telegraph wires. The interesting paper of Mr. G. Preece gave a most complete account of probably the most perfect underground telegraph line ever laid down—at any rate, the most perfect one ever constructed in this country; but we heard on that evening very little as to the result of the history of the past maintenance of underground telegraphs, or of the experience of those who are connected with their construction. I then ventured to remark that the history of underground telegraphs, so far as we know, was

not such as to make it desirable to perpetuate the use of covered wires carried underground; but I only did so in order that perhaps others who had had experience of it in other countries would tell us something about it.

I have been called upon by the President to say something on this subject, and I have risen in obedience to his call; but I think the paper we are going to hear this evening is likely to be of so much more interest than anything I could say on underground telegraphs that I will make no further remarks. I have no doubt this will be matter of future discussion in this Society; and from what I have heard from gentlemen who have known what underground telegraphs have done, and how they have been used, and their failures and successes, it is not the last time we shall hear of the matter.

Mr. CULLEY: The great difficulty with underground telegraphs, as far as I know, has been in the joints. Mr. G. Preece took especial care with his joints, and they appear to stand very well indeed, but it is almost impossible to afford the time for making the joints in the perfect manner Mr. G. Preece has done. We have at the present time from 250 to 280 gutta-percha-covered wires running through the Holborn Viaduct. To make the joints of those 280 wires would take a long time, but the rough mode we have adopted, in competent hands, is fairly successful. We have not so high insulation as Mr. G. Preece obtains, but still it is very fair. Although we try to teach the men to joint, we find our wires give us more or less trouble at the joints, not, however, so much in London as in the country, inasmuch as the work in London is done generally by men more experienced. If any one could invent a joint in gutta-percha wire which could be done more quickly than the method of uniting the gutta-percha now used, and give us a fair joint and a fair amount of insulation—say ten million ohms resistance—if it could be done quickly, so that a man could join up (say) sixty wires per day, it would be a great advantage; and, perhaps, before we discuss this subject again, some Member of the Society may hit upon a method. It is one of the things which wants to be done.

Mr. TRUMAN: The last *speaker* has said it would be a great

advantage to the Society if we would produce a joint which could be made quickly and could be relied on. I think I am in a position to say such a joint can be made. I mean a joint can be made of perfectly solid gutta-percha, without any loss of continuity whatever, between the covered wire on the one hand and the covered wire on the other; and I think such a joint could be made in the course, perhaps, of twenty minutes. I speak with diffidence in a Society composed of gentlemen who know so much more of these matters than I do; but I have had some experience in the use of gutta-percha, and for many years it has been my hobby to employ all the knowledge I had, for the purpose of advancing the use of that material for telegraph purposes, and one of my main objects has been to improve the joint, and I believe the joint can be made absolutely solid. But I think one of the great impediments in the way of the perfect joint is the compound of materials. We cannot manipulate with gutta-percha properly when we have another substance intervening between the layers of gutta-percha. I recollect Mr. Statham some years ago speaking to me on the subject of gutta-percha, as then used—that was previous to 1860—and he stated that his friend Gisborne told him that he believed gutta-percha would never be improved by the addition of any other substance to it, but that the gum in its purity was the material to which we must trust for the insulation of our great telegraph system. That quite agreed with my own feeling on the matter, and that suggestion led me, in the first instance, to endeavour to produce as pure a material as we could get. That investigation has led me to this—that I believe solid gutta-percha covering, without any compound between it and the joint made of gutta-percha solid, is the way in which we shall arrive at what we want. I would simply say at some future period, when this discussion is raised again, I shall be happy to lay before the Society any fresh knowledge I may have gained on the subject of joints.

The PRESIDENT: We all know the vital importance to telegraphy of the proper answering of this question about joints, and I have listened with interest to the statements just made by the two gentlemen who have spoken. If no other gentleman present has any information to give or remarks to offer, we will proceed with the

paper which stands on the list, and I will call upon Mr. Culley to read his paper " On Duplex Telegraphy."

AN ATTEMPT AT A FAMILIAR EXPLANATION OF THE DUPLEX PRINCIPLE.

By R. S. Culley, Vice-President.

I have found, from my own personal experience, that it is a very difficult matter to SEE the principle of the duplex system.

The first questions which are asked are, How can the currents pass one another in the line wire? and, if they do pass, how is it they do not interfere one with the other?

Telegraphists are but too well aware that, when two stations dispute for the possession of a circuit, the signals of each are affected by those of the other. If the instrument is a single needle, it is found that, when the direction of the two currents is the same, the needle moves more strongly, and when the direction differs the needle scarcely moves at all. The fact that the effect of the current received from another station is visible, even while one is signalling, is therefore familiar. This effect is entire confusion of signals; yet let us see whether it would not be possible to unravel this confusion.

Suppose then, first, that each sender knows the force with which his needle should strike the stops, by the action of his own current. Station A can then see (I mean it is possible for him to see) when the distant station B sends a current in the same direction as his own, or tries to make his needle move to the same side; because his needle (A.'s) strikes the stop more strongly. Let A then read this as a movement similar to his own.

Secondly. When B tries to move A.'s needle in the opposite direction to which A himself moves it, it strikes the stops less strongly, or perhaps moves only half-way to the stops. Let A read this as a movement in the opposite direction to his own.

I of course do not mean to say that *practically* one could read in this way, but it is not absolutely impossible to do so. We will say, then, that A can with great care read B's signals, while signals are being sent very slowly by A himself. And if A can read B, of course B can read A, for what has been said with respect to A applies equally to B.

And it may be seen that if by any means we could arrange so that neither station's own or outgoing current should affect his needle, leaving the dial free to show only the effect produced by the incoming current, the difficulty of reading would vanish.

How then can each instrument be so connected up that neither sender shall move his own needle, and yet so that the coils shall always remain in circuit?

I have taken the needle instrument for my illustration, because we are all familiar with it. We are all more or less familiar also with the process of testing for resistance with the differential galvanometer. Let us consider what occurs in testing a line-wire between two stations for resistance with the differential galvanometer. Until the resistance of the rheostat (or resistance coils) has been made equal to that of the line, the currents sent by tapping the key attached to the galvanometer move the needle, in the one direction if the resistance of the rheostat is too small, in the other direction if the resistance is too great. But immediately a balance is obtained, or, in other words, when the two resistances have been made equal, currents or letters sent by the key at A do not move the needle at all, because the current divides equally between the coil connected to the rheostat and that connected to the line, which two coils, being wound in opposite directions, counteract one another. We will suppose that B's ordinary instrument has been left in circuit, and that, when A commences to test, B interrupts by sending occasional signals; these signals will, of course, be visible on A's differential, but will be mixed up with the currents A is sending in order to obtain a balance. But directly a balance has been obtained, for we will suppose B's currents have not been sent so perseveringly as to prevent A making his test, the incoming currents, or rather the incoming signals, will be perfectly separated

from the outgoing, and whether A now sends + or — testing currents, and whether they are sent slowly or fast, he will be able to read all that B may say.

I have here two single needles wound with two parallel wires, like differential galvanometers—they have the drop-handle keys. We will suppose this one (A) to be a differential galvanometer.

I will now attempt to test the wire and apparatus at B by the differential. I will send currents and adjust the pegs of my resistance coils to obtain a balance. B interrupts now and then; his signals are of course visible on my dial, and interfere with the movements of my needle. My testing current moves my needle to the right, B sometimes moves it right and sometimes left.

He however gives me time to adjust my resistance coils, and now I do not move my own needle, because I have succeeded in making my coils equal in resistance to the line and B's instrument. Whether I move my handle or not, B's signals are now quite legible to me. My current does not act on my own needle. But B cannot read my signals, for they are confused by the currents he is sending.

B now converts his needle instrument into a differential, by putting in circuit the second wire with which his coils are wound, and adjusts his resistance coils. We saw that before he had done this all was confused, but now that the balance is obtained my signals alone appear on his dial, his own have no effect on his needle.

Thus, so long as the resistances of the rheostats at each end remain equal to that of the circuit tested, each station will see the currents sent by the other, although neither of them can see his own.

The reason is this, the currents *sent* by each divide equally between the line and the rheostat, passing through the galvanometer coils in opposite directions, and therefore have no effect upon the needle of the sending instrument. But, when the distant station sends a current, it either aids or opposes the home current; in the first case it adds its force to that portion which passes through the coil connected to line, so that more flows to line than to rheostat and the needle moves; in the second case it diminishes the current

passing to line, more now flows through the rheostat, and the needle moves in the opposite direction.

It will then be seen that the two currents do not pass one another, as has been imagined, but that, when both stations signal at the same time, the current sent by either station acts upon the distant instrument by determining whether the currents sent by that station shall pass through the line or the rheostat.

To take the simplest case, that of a differentially wound needle-instrument, worked by a single current, so that the needle is moved in one direction only; the batteries being so connected that when the keys at both stations are pressed at the same time the currents flow in the same direction and assist one another.

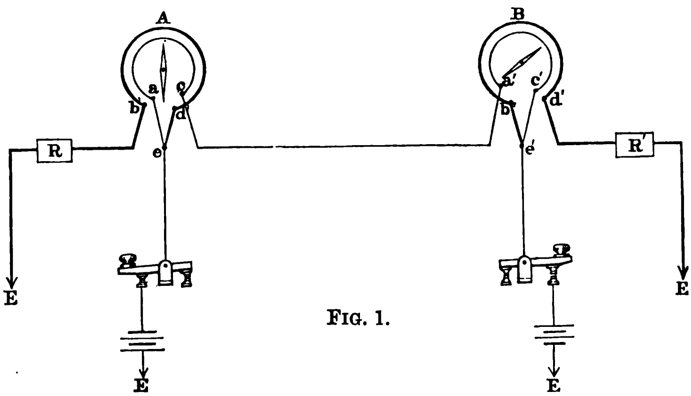


Fig. 1.

(*a.*) When station A signals separately, the current is divided at *e*, and its effect balanced in the coils *a c* and *b d* of the home instrument, but it passes through both coils of the distant instrument in the *same* direction, entering at *a′*, passing from *c′* to *b′* through the junction *e′*, and to earth by *d′* and the rheostat R′, and therefore produces a signal.

(*b.*) If both A and B depress their keys at the same moment, the two batteries are added to one another so far as the line wire is concerned; this produces an effect on the differential arrangement at both ends equivalent to a lessening of the R of the line, and therefore more current flows to the line than to the rheostats.

In the needle, as in the double-current Morse, the actions are more complicated. We have one sending +, while the other sends −; both may send +, or both −.

When A sends a + current, while B sends a −, the two are in

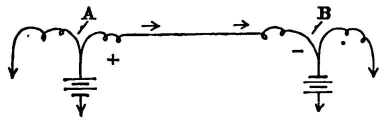


FIG. 2.

the same direction, and both needles move alike. We know that if we place batteries at both ends of a line, and connect copper to earth at one end and zinc at the other, we obtain a stronger signal on both instruments than if but one battery were used; and we can thus see why, in this case, more current passes to line than to resistance coils.

When both send + the batteries oppose one another, and we know that, if the opposing batteries be equal in force, no signal

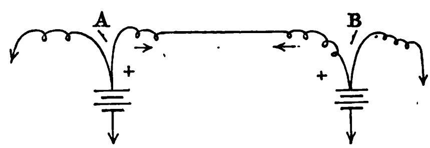


FIG. 3.

will be produced at either end; but the current passes freely from the home batteries through the rheostat circuits, and both needles move in obedience to the coil connected to those circuits, and of course in opposite directions. When both send −, a similar effect occurs, but the needles move the other way.

Let us now consider how the phenomena may be explained by the law of difference of potential. We know that a current flows from a point of higher to that of lower potential, and that, where no difference of potential exists, no current can be set up. Moreover, that if a point of high potential be connected by two circuits of equal resistance to two points, one of which is at a lower potential than the other, the current will be strongest in the circuit connected with the lower of the two potentials.

Consider the case in which A sends +, B −. The line and the

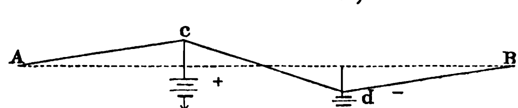


FIG. 4.

rheostat circuits are equal in R. Set off three equal resistances on the datum line, the zero of potential; *cd* will be the R of the line and line-coil of each instrument; A*c* and *d*B the R of the rheostats and coils connected with them. Here the difference of potential between *c* and *d* is double that between *c* and A or *d* and B; therefore the current will be stronger in the line coils than in the rheostat coils, and the instrument will obey the former.

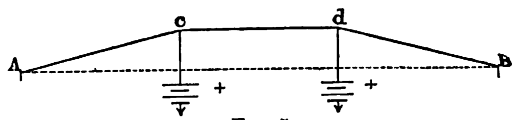


FIG. 5.

Let both A and B now send +, the zinc pole of each battery being to earth. Here there is no difference of potential between the two ends of the line *c* and *d*, and therefore the current passes by the rheostat to earth. The insulation of the wire is supposed to be perfect in these cases; if it were not perfect, the resistances A *c* B *d* would have to be decreased.

DISCUSSION.

The PRESIDENT: Has any gentleman present any questions to ask? as I am sure Mr. Culley will add to the interest of what he has stated by answering such questions.

Mr. LATIMER CLARK: I have listened to the paper with great interest, but I have been surprised that no mention has been made of the history of duplex telegraphy. I think the name of Stearns was mentioned as having introduced the system which has been described this evening, but it is only right to say the duplex system was known as long ago as 1853, when experiments were made by the Electric Telegraph Company, and answered tolerably perfectly; but at that period the business was not sufficient to render the acceleration of messages necessary, and it fell into disuse. About that time Messrs. Gintl and Siemens made improvements in the system, and no doubt, if the same demand for quick transmission existed then which there is at the present time, it would have come

into practical operation and general use. It was however laid aside and forgotten, having been looked upon as a scientific telegraphic curiosity, till it was recently revived in America, and Mr. Stearns has worked at it both here and in America, and to him is due the credit of having added to the system the use of condensers. As is well known, in its incipient stage we only thought of balancing the resistance of the line and battery; we did not go far enough to discover the balancing the resistance by means of induction. Mr. Stearns has observed the effect of induction on the line, and has shown how to obviate it. The system is very important in the present day. I learn from Mr. Culley that it is coming into extensive adoption in this country. I know it is so in America, and I think it probable it will soon be universally used throughout the world. I believe it will be used both for land lines and submarine; in fact the continuous experiments of the Eastern Telegraph Company have shown practically it can be applied to submarine lines, and the practical result will be that it will not only double, but more than double, the sending-power of the line.

Mr. Culley: No doubt the system is a perfect success on distances up to 200 miles; but our climate is so capricious and bad for telegraphy that I experience occasionally a difficulty in working distances of 400 miles. Up to 300 miles there may be little or no difficulty, but beyond that there are difficulties on exceptionally unfavourable days, and they may perhaps amount in the aggregate to a couple of weeks in the year, that is to say, 14 days out of the 365; but probably before another year is over we shall have overcome that difficulty.

The President: If no other Member has any remarks to make, I will, before we thank Mr. Culley for his paper, ask permission to say a few words, and they shall be very brief. I have listened with great interest to the account which Mr. Culley has given of the successful introduction of the duplex system. Mr. Stearns's method contains one element in addition, not previously, so far as I know, published, for working on the duplex system. Mr. Culley has explained the introduction of the condensers, and I believe we may quite understand it is this point which has rendered the duplex

system practicable in the more considerable lengths on which it has recently been tried with success. This is an essential point no doubt. So far as the general principle of the introduction of the condenser is concerned, it is a rudimentary development of a method long known for the *direct* working of submarine telegraphs. That consists in the application of a complete model of the submarine cable in connection with the rheostat resistance at the sending end. The conditions which have been explained by Mr. Culley require, that, when the operator places his key, precisely the same effect shall be produced in the secondary or adjustment line as is produced in the main line; I mean, precisely the same current should run through the key into the secondary line as into the primary line. To make sure that not only shall the whole amount of the current be the same, but that it shall commence at the same moment, rise with the same rate of augmentation, and come to nothing again at the same rate—in short, in order that the power represented by one current shall be the same as that through the other side—a complete imitation, or model, so to speak, of the main line, in the adjustment arrangements, must be made. When I say equality is necessary, I ought to correct that, and say, proportionality would suffice, but equality was the most convenient system. Some of the old methods consisted in giving proportional currents—currents which, though not equal in the two branches, bear a definite proportion one to the other, and the same with reference to the refinement of producing an artificial submarine line in the adjustment arrangements. The idea of getting a perfect balance by Varley's artificial line has long been known, but whether it is actually valuable or not I am not prepared to say. I make that remark not at all with a view to take anything from the great credit due to Mr. Stearns for having introduced the one thing to make duplex telegraphy practicable.

With reference to successful duplex telegraphy in submarine lines, this has been solved for the first time by Mr. de Sauté on the line between Malta, Gibraltar, and Lisbon, and the result was certainly very striking and interesting. You have read the account of those experiments which has appeared in the Society's Journal, and will recognise the more complete working out of the thing

which is essential to give success to duplex working, in comparison with the rudimentary use of the condenser which Mr. Stearns has made so successful in the case of land lines. There are, however, two characteristic difficulties of the two applications—the one affecting land lines, and the other not spoken much of as affecting submarine lines. The frequently and often rapidly varying insulation on land lines renders it difficult to keep the adjustment correct, to change it quick enough, and make the adjustment frequent enough to allow the action to go on notwithstanding this variation of insulation. On the other hand, in submarine lines, on good lines, there is perfect insulation; and further, the balance required for a submarine line is very much finer than what is required for a land line. The difference between the strength of current going into the line from the sender's instrument and the exceeding attenuated changes of strength which occur, by which alone the messages are read off in long submarine lines, presents to us evidences of very great difficulty, so far as duplex working is concerned. It is therefore greatly to the credit of the operators on the Eastern line that they have succeeded so well as they did in the experiments referred to.

Any amount of adjustment to be practically perfect in the working of the duplex system on long lines can only be answered by actual working. The theory is obvious enough, but the practice is difficult, even on so short a line as that between Gibraltar and Lisbon; and that has fixed the attention of Mr. de Sauté and his colleagues there. It is an interesting point, and I hope that they will not flag in their efforts, but that they will find out what can be done on the longer lines, and show at what rate of signalling the duplex system can be worked on the longer submarine lines. If the lines could only be worked slow enough, the signals are those of a perfectly insulated land line, and therefore what can be done on a land line can be done with greater assurance on a submarine line if speed is not an object. But the great object is to increase the speed of the lines. If, on the other hand, the adjustment can be made successful for submarine lines of great length, working at something more than half the speed at which it can be usefully worked on the

single system will be useful. As Mr. Culley has pointed out, it more than doubles the speed, from not having to wait for clearing out the line, and never interrupting the sender at the other end. That is the practical view of the subject, and for the solution of the question we must look to the operators on the great submarine lines; and I hope before long we shall have further information upon it. I beg to tender the thanks of the meeting to Mr. Culley for the valuable communication he has given to us this evening.

You will recollect we announced the business to-night with the continuation of the discussion on underground telegraphs. I had not the pleasure of being present when the paper was read, but I am sure it is our duty as well as pleasure to give a vote of thanks to Mr. G. E. Preece for the paper on that subject, which he was good enough to read on a former occasion.

Mr. Latimer Clark: I have great pleasure in seconding the proposition of the President. I was absent when the paper was brought forward, but I read it at home with much interest, and we are much indebted to Mr. G. Preece for writing it. The great value of a paper of that kind is not so much what it teaches as what it suggests. I hope it will set all the Members thinking, and trying to introduce some improvement in the art of laying wires underground. Any one who looks at our railways with posts on both sides, carrying a heavier burden of wires than is safe for them to do, and any one who considers the wonderful increase and growth of the telegraph business of the country, the magnitude it has attained, and the development we may expect hereafter, must see that, unless those instrumental improvements such as we have been discussing to-night, or some other means of sending messages quicker, are adopted, we shall soon find ourselves with more business than the wires can carry, with no more places to put fresh wires. Therefore, it is a matter of great importance, and it is the duty of Members to study carefully, the best means of laying a great number of subterranean wires in a safe and permanent manner. I have been surprised to find how little discussion there has been on the subject. I have been surprised that Members of great manufacturing firms and companies have not spoken upon it, for their interest in the subject must be very deep indeed. I agree with my

friend, who said he trusted we should have another paper on this subject, and I hope when that is done it will be more fully discussed.

The following candidates were balloted for, and declared duly elected:—

As Associates.

A. R. GRANVILLE . . .	Barnsbury.
W. A. NEWNHAM . . .	Indo-European Government Telegraphs, Persian Gulf.
WILLIAM MORRISON . .	Post Office Telegraphs, General Post Office.

As a Student.

C. H. PHILLIPS . . .	Victoria Park.

The meeting then adjourned.

The Twenty-second Ordinary General Meeting was held on Wednesday, 11th February, 1874, Dr. C. W. Siemens, F.R.S., Past President, in the Chair.

The following paper was read by

Mr. Nath. J. Holmes

ON

"THE APPLICATION OF ELECTRICITY AS A MEANS OF DEFENCE IN NAVAL AND MILITARY WARFARE."

The importance of a well-constituted system of electric torpedo mines, as a means of defence against the approach of an enemy by sea or by land, is now recognised by most governments.

When properly constructed and laid down, and placed in the hands of skilful operators, rifled guns and armour-plated vessels should be powerless for attack against the destructive effects of the electric torpedo defence.

In the report of the Secretary of the United States Navy, published as far back as December 1865, when the torpedo system was only in its infancy, and manipulated by the Confederate engineers under every possible disadvantage, it is stated "that when the United States fleet attacked and passed the forts erected for the sea defences of Mobile and of Wilmington, mounting together nearly six hundred guns, many rifled and of the heaviest calibre, the only vessels lost by the United States Government in both these attacks—and the shore batteries of the Confederates were splendidly served—were destroyed by *electric torpedoes*, which, always formidable in harbours and internal waters, have proved *more* destructive to our naval vessels than *all other means combined.*"

Too much attention cannot therefore be directed to the perfect development of this comparatively new system of warfare, com-

bining, as it does, economy in cost, efficiency in action, safety in working, security in defence, and devastation in effect.

The experience resulting from a study of the tactics of attack and defence during recent wars demonstrates that perfection in the mechanical appliances for the attack, or the defence, is equivalent to the effective strength of numerical numbers in the advantage gained over the enemy; and, as mechanical appliances can be obtained at a less cost than the maintenance of a standing reserve, the defence of a kingdom resolves itself in great part to the establishment of properly applied scientific systems, controlled by intelligent operators, as against the too often lavish expenditure of revenue in the construction of unwieldy armaments of guns and armour-plated ships competing for mastery in the never-ending contest that the increased strength and power of each continually provokes, without any very definite notion as to whether the balance of power remains in favour of the rifled gun or iron-clad.

With the electric torpedo this competition ceases. Simple in its construction and action, called upon to expend its power upon the enemy without reciprocal challenge, the most powerful Monitor or armour-plated ship ever constructed easily falls a prey to its deadly embrace. At the cost of a few thousand pounds the strongest ship with its heavy guns and gallant crew, fitted out at a cost of hundreds of thousands of pounds, becomes comparatively inoperative for the attack; half-a-dozen men in control of the torpedo mines can effectually keep at bay both an army and a fleet, and at will, should either encroach within range of their "area of destruction," annihilate in a few seconds the advancing foe, who, if not totally destroyed, will be at least fatally crippled.

One illustration of the effective power of a well-planned torpedo mine will serve to demonstrate the value of this defence.

During the American civil war the important defence of the water approach to Richmond was intrusted to a single electric torpedo mine sunk in the channel-way of James River. This mine, of considerable power, was under the control of an officer, who, stationed on one of the river banks, watched, from the sand-pit where he lay concealed, the approach of the enemy. A single stake planted upon the opposite bank served to indicate—by the

passing vessel being in a line with his station and the stake—the exact moment when she would be within the area of destruction. With the patience of a spider watching for its victim, so for thirteen months did this officer remain waiting for the opportunity to explode the mine with effect. At length the Federal fleet, under command of Commodore Lee, entered the James River — the Commodore's vessel being the third in the advancing rank.

The foremost vessel, carrying seven guns, and manned with a picked crew of one hundred and twenty-seven men, was allowed to pass over the mine in safety—it being by arrangement held in reserve for the Commodore's ship—when the order having been passed from the deck of the next vessel, and audible on the shore, for her to fall back and drag for torpedo wires, the officer determined to explode his mine, and "hoist" her as she descended the stream. The explosion took place upon a clear afternoon, and was witnessed by several persons; the hull of the vessel was visibly lifted out of the water, her boilers exploded, the smoke-stacks carried away, and the crew projected into the air with great velocity; out of the hundred and twenty-seven men only three escaped alive — the vessel was literally blown to atoms, or, using the American phrase, into "toothpicks."

The awfully sudden and unexpected destruction of this vessel paralyzed the operations of the Federal fleet for a time, and Richmond was saved; Commodore Lee, declining to advance, sunk several of his ships blocking up the channel-way. This obstruction afterwards, on the advance of General Butler, gave rise to the cutting of the "Dutch Gap" canal, now a matter of history.

Destructive as are the effects of torpedo explosions, there is little room to doubt that the introduction of science into naval and military tactics exercises a most wholesome effect on the moral physique of the men, accelerating the termination of a war, and is also a deterring element in the political exigencies which would hasten such a conflict. What admiral would willingly lead his fleet into waters known to be infested with deadly volcanoes from which there is no escape, any explosion being synonymous with certain destruction? What general would march his army over ground suspected of being mined in all directions with treacherous

pitfalls mathematically laid down and concealed under cover of verdant pastures and a smiling landscape, in each case devised to explode only when the advancing foe is within their fatal grasp?

Whenever electric torpedoes are employed as a means of defence against attack, it is essential that the system used should be manipulated with accuracy and security, and that neither danger to the operator, or failure in the mechanical effect of the ignition, should attend the discharge of the mine when the defence has been properly constructed and carried out.

To render any system of electric torpedo defence practically useful and reliable, it must be under the perfect control of the officers and men using that system, and who rely upon the destructive effect of the torpedo for the protection of their position, in place of guns and armour-plated ships.

The torpedo to be used as a defence in warfare must be reduced to a system defined in its principles and practical in its applications; and, like the practice of gunnery, become a recognised branch of military and naval instruction.

It is likewise necessary that the electrical arrangements shall ensure the ignition of the torpedoes by land and sea at distances exceeding that of the effective range of cannon, and that when necessary more than one mine shall be exploded in group simultaneously on the same circuit.

The following points may therefore be enumerated, as the essential conditions of a torpedo system of defence:

First.—The torpedoes in themselves must be non-explosive and harmless, not liable to accidental discharge by percussion or carelessness.

Second.—The power of testing at all times the submarine or land circuits of the torpedoes without danger of explosion, and of speaking and telegraphing information and instructions through the charge without risk.

Third.—The power of igniting the mines at will, and of discharging several torpedoes in group with a single wire, at distances exceeding that of the effective range of artillery, and that the explosions shall only take place when the vessel or vessels to be destroyed are within the area of destruction.

Fourth.—The power of discharging the mines, even should the enemy succeed in breaking one of the conducting wires, and of preventing the explosion of the mine by the enemy.

Every torpedo in its complete form consists of three parts: the ignitor, the charge, and the torpedo case or tank, together with the necessary internal and external arrangement of electric connections and conductors, giving the operator the entire control of the mine. The destructive power of both the land and sea torpedo will of necessity depend upon the amount of powder or charge placed within the mine, the conditions of the attack, the effect to be produced, and proper attention to the various important details connected with the electric circuits, laying down, testing, and ignition of the mine. When the necessary precautions are observed in connection with these details, all danger of involuntary explosion is removed, and accidents become impossible even in the hands of inexperienced officers.

The power of testing the effective condition of the circuits and connections within the torpedoes, and of speaking through the mines without danger of ignition, so as to maintain telegraphic communications between the several outlying stations and the centre of action, constitutes one of the important features of the Holmes and Maury system, developed by them in 1863 during the civil war in America, and one which has more than any other contributed to inspire that confidence in the submarine and subterranean torpedo which the value of the invention demands.

The importance of this result cannot be over-estimated when the value of the telegraphic communication in active service is considered.

The experience of the victories by the Prussian armies in Bohemia points to the great importance of maintaining telegraphic communication between the outposts, stations, and out-lying divisions of the army in regulating the successful issue of their military manœuvres, and the same rule applies equally to naval tactics.

For instance, let it be supposed that the enemy's fleet is advancing up channel; with this system orders could be immediately transmitted from A Station to B Station, directing attention to such and such group of mines, under certain instructions to be

given during the progress of the attack, or the emergencies of the moment; and the transmission of such intelligence, while it places the whole field under the control of the commanding officers, at the same time points out the integrity of the several electric circuits.

The recent Franco-Prussian War likewise affords a very instructive example, illustrative of the inefficiency of a divided attack or defence in the absence of special telegraphic communication.

The well-appointed and expensively maintained French naval force sent to the Baltic, to carry out concerted operations by land and sea, to effect a diversion in favour of a land attack upon the enemy, was practically useless and inoperative, from the absence of telegraphic communication from head-quarters directing the manœuvres and organising the diversion; thus a most valuable opportunity for an effective land attack along the Prussian frontier, coincident with a naval engagement off the southern shore of the Baltic, of immense importance to France at that critical time, was absolutely and entirely lost, and all joint and reciprocal action between the French army and naval forces completely frustrated.

The ignition of the electrical mine has next to be considered, and upon the certainty and accuracy of this operation depends entirely the value of the torpedo as a means of defence; delay or doubt for a moment upon this point might prove fatal to the prospects of victory.

The importance of this accuracy and precision of ignition at sea will be understood by calculating the length of time the enemy remains in the line of vision. A vessel steaming, say at the rate of nine knots an hour, will move through the water at the rate of 18 feet per second, and, supposing her length to be 300 feet, she will remain in position to receive the effects of the blow only sixteen seconds, scarcely a quarter of a minute.

These several points being well considered, that necessary control and security are obtained in the manipulation of the electric torpedo, so essential to render it a safe and reliable defence, and one of the most deadly and destructive engines of modern warfare that the inventive mind of man has as yet devised.

Thus one of the essential conditions of every torpedo defence is,

that when called into action it shall fulfil the objects for which it was designed, whether this be as a defence against attack by land or by sea, either singly or in conjunction with that of heavy artillery.

It becomes therefore a matter of vast importance to carefully investigate the conditions under which the defence is established; if by sea, the nature of the bottom on or over which the mines are to be placed—the depth of water, the set of the tides and currents, and the strategical positions to be defended. If by land, the probable nature of the enemy's attack and advance requires consideration, and the successive positions to be maintained.

In every system of electric defence, the attention and consideration of the torpedo engineer should first be directed to obtain an accurate knowledge of the ground, whether rock, sand, or mud,—the currents, depth of water, and rise and fall of the tide over the area in which it is intended to carry out a torpedo system.

The importance of ascertaining the nature of the bottom is at once apparent, for, if found to be rocky, special arrangements must be carried out to secure the immoveability of the mine in the position originally assigned to it, any deviation by reason of currents dragging over the bottom being absolutely fatal to the effective discharge of the mine for destructive purposes. Again, if the bottom is sandy, then careful investigation requires to be made as to the stability of the sand; should it prove to be of a shifting nature, every precaution must be taken to properly calculate the strength of the conductors so as to prevent breakage by undue pressure, either by the wires being silted over, or by being underswept by the action of the current, and exposed to an unequal strain. Again, if the bed of the ocean or river should prove to be of a yielding nature, such as mud, the mine might become buried, and the calculations as regards the effective force of the explosion be materially diminished in relation to the column of water between the mine and the object of attack.

It must be borne in mind that these points are essential in every system, as torpedo mines may be months submerged before called into action. Such was the case with the James River mine, which lay thirteen months in the bed of the river before called upon to display its destructive properties.

Again, a careful estimate of the strength and direction of the surface-currents and tides is equally essential, because in discharging a mine calculation has to be made for the swiftness of the motion of the vessel. When only seconds are allowed for the "effective shot," it is certainly a matter of moment for the operator to be well informed of her velocity, moving either with or against stream. The utmost nicety of calculation and manipulation is required to insure an accurate and "decisive" explosion. No gunner in command of a battery can expect to aim with accuracy unless he has a practical acquaintance with the principles which regulate the flight of projectiles. Constant practice alone enables him to obtain the command of his gun at the distances at which he is called upon to operate. So with the torpedo mine, practice alone can impart to the engineer the necessary confidence in the conditions under which the ignition is required to take place. With the gun the action of the wind upon the flight of the projectile in relation to the strength of the charge enters into the elements of the calculation. With the torpedo engineer the velocity of the current and the depth or cushion of water form an equally important feature in the effective manipulation of the mine. The depth at which torpedo mines are submerged below the surface is again of vast moment in relation to the strength of the charge and bursting power of the case. Water being for all practical purposes considered as incompressible in every direction, it becomes evident that the effective action of the torpedo mine will always be that of the path of least resistance, or, in other words, in a vertical direction; but it does not always follow that this is practically the case should the power of the charge and the *vis inertia* of the resistance be improperly calculated. It is on record that Admiral Chabannes, when trying some experiments with submerged electrical mines from the jetty at Toulon, found that the effective force of the mine was conveyed along the bed of the ocean, and, the force of the explosion being thus transferred to the piles of the jetty, the operating party on the pier were knocked down, or rather hoisted by their own petard, while the surface of the ocean, and the "area of destruction" over the mine, remained perfectly tranquil and powerless to destroy the enemy supposed to be in position over it. In this instance the strength

of the charge in relation to the depth of water or resistance to be overcome had been miscalculated in relation to the conducting or vibrating power of the bed of the sea upon which the mine was placed—its distance from the piles of the jetty had not been properly considered. In fact, the depth of water being too great for the charge, and the resistance of the intervening ground between the mine and the jetty being less, the effective power of the mine, taking the path of least resistance, found vent for its energy through the vibratory motion it communicated to the ground, culminating in the discomfort of the Admiral and his party assembled on the Toulon jetty to witness the effect.

The foregoing remarks will have demonstrated the important part electricity, when properly applied, is destined to play in future naval and military operations. At present the value of this system of defence against attack is scarcely recognised; were this so, there would not be such an amount of money expended in vain attempts to introduce elaborate mechanical contrivances, in most instances totally unsuitable for active service in heavy sea-ways and tides. The too frequent misapplication of inventive genius is much to be lamented, as the steady advancement of an established system of electric defence as an auxiliary to our naval and military armaments is thereby retarded. It is only necessary to refer to the ill consequences of relying on mechanical means in warfare to show that these remarks are well founded.

On a certain day during the American civil war General Grant had planned that the investment of Richmond should be made.

A mechanical mine had been laid, calculated to explode at a given hour, and upon the springing of this mine the assault was arranged to take place.

This mine did not go off at the appointed time, and the assault did not take place, and Richmond was saved. Had this mine been under electrical control the explosion could have been calculated to the precise moment when General Grant's forces were in readiness to follow up the attack. The uncertainty of the ignition of this mechanical mine, which did not take place until some hours afterwards, frustrated the entire operations of the land attack intended to have been carried out against Richmond.

Another most instructive lesson, as regards the employment of mechanical mines in warfare, is afforded by the Prussian defence of the mouth of the Elbe, in the recent Franco-Prussian war. The navigation of this river was closed by the use of mechanical torpedoes, and closed not only against the approach of the enemy, but also against the entrance of their own ships, and the lamentable loss of life to the Prussian engineers in the manipulation and placing of these mines is of too recent a date to be forgotten. Not only were the engineers exposed to great danger in the handling of these mechanical volcanoes, but at the termination of the war, when it became necessary to remove all such obstructions to navigation, the frequent recurrence of fatal explosions should act as a caution against the introduction of any such system into the appliances of modern warfare.

Not only may every mechanical system of torpedo defence, either by land or sea, be denounced as dangerous to the officers employed to manipulate the same, but from their necessarily uncertain action liable to prove most disastrous in connection with any organised or concerted system of land or sea attack; and too much stress cannot be given to enunciate the importance of the employment of the electrical agent in all our naval and military armaments, wherever practicable, over the introduction of mechanical toys that at present too often occupy the attention of naval and military departments throughout Europe.

The practical application of the electrical agency as a means of defence may certainly be dated from 1864, the period of the recent civil war in America; and, disastrous as its application was to the interests of the United States fleet, the American Government have fully recognised the electrical agency as one of the most potent means of protecting their naval and military stations from the approach of any hostile force.

In carrying into effect the laying down of any system of electric mines for a defence it is of the first importance to observe secrecy in the details of position and the area over which the mines are placed.

The demoralising effect produced upon Commodore Lee's fleet in the James River by the destruction of the "Commodore Jones,"

in ignorance of the extent to which the bed of the river had been mined, frustrated the advance of the fleet to Richmond. Too much stress therefore cannot be placed upon the value of secret service in the organisation of every electrical defence. As there are various systems of arrangement and detail of manipulation in connection with the circuits and mode of igniting the charge which may be employed, it is not intended in this paper to advocate any special method; the nature of the channel, tideway, depth of water, sea-bottom, and land approaches, will of necessity to a certain extent regulate and suggest to the torpedo engineer the means best calculated to ensure successful operations against the advance of the enemy.

It is, however, within the purpose of this paper to suggest such a system as may generally be found efficient. To insure accuracy of discharge of the mine at the precise second of time when the enemy's ship is over the mine, as exemplified by the diagram, the concurrent observation of two stations is essential, and also the establishment of a base line of communication between these points of surveillance.

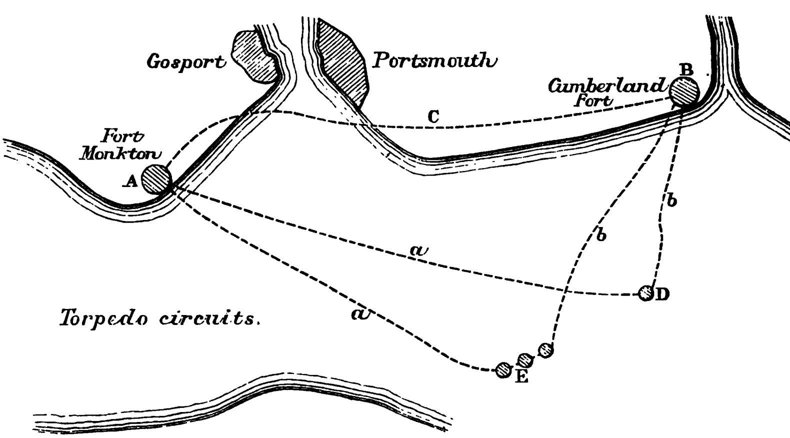


DIAGRAM OF PORTSMOUTH.

In speaking therefore of electric warfare in this paper the system is regarded in the light of an established national defence, auxiliary to that of armour-plated vessels and gun-boats, rather than a

defence of an impromptu nature, and advocates the establishment of permanent torpedo forts and fixed shore points of observation as an essential condition of every national defence.

In illustration of this principle reference may be made to the sea-approaches to Portsmouth Harbour, the position of the permanent forts by land and sea, and the nature of the channel, being, as it were, specially planned by nature for such a defence. The base line of operation between the two stations A and B (*see diagram*), an essential element in every well-considered system of electrical defence, insures telegraphic communication to be maintained at all times between the two forts Monkton and Cumberland.

The importance of this base line of operation will at once be understood by reference to an advancing foe. It is not by the wholesale destruction of an enemy that victory is gained; a judicious coup, a well-planted blow, will destroy the nerve of the enemy. In torpedo warfare we have so far no precedent but that of the American Civil War of 1864. Our practice and experiments are therefore mere fine-weather illustrations; the stern realities of action and destruction of life and property, consequent upon the introduction of this system into modern warfare, are so far spared our country.

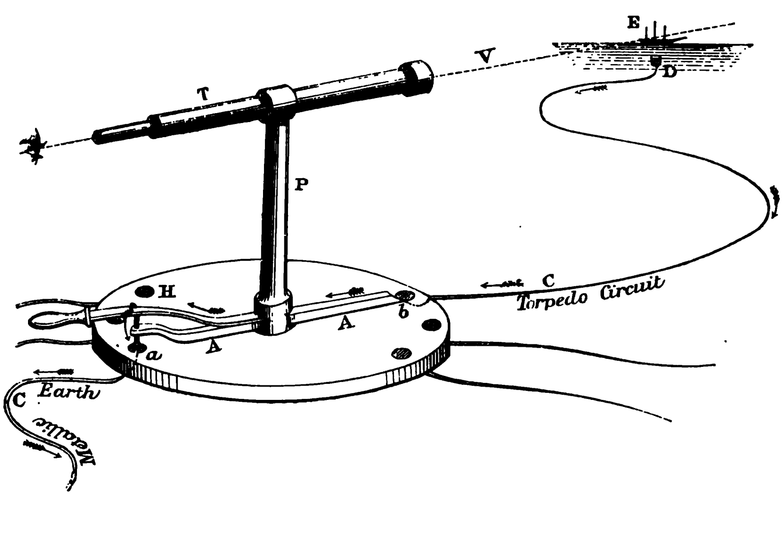
Let me therefore refer once more to the water-defence of Richmond: one torpedo well planted sufficed to close the James River to the Federal fleet; consequently, with an advancing enemy over a torpedo way, one operation skilfully carried out may, with little expenditure of the vitality of the defence, be found sufficient to destroy the tactics of the advancing foe.

Unlike the employment of artillery in warfare, where each piece can again be brought to the front after successive discharges, a torpedo mine once ignited when the action is on, and non-effectual in its results, may be considered as a victory gained by the enemy; that position is lost and becomes a safe standing-ground in the advance for the attack. It is therefore apparent that every precaution to insure accuracy must be initiated with the defence.

The accompanying diagram shows in elementary outline the system to be observed in taking concurrent observations from the

several-base line positions. The angle of action for each successive range of mines having been marked upon the vernier at the time of the submergence of the several mines, as the enemy's vessel comes into line at each station, with these set angles of observation, it is obvious that unless by concurrent observation at two stations the enemy is in position for destruction with both stations at the same moment of time (we are dealing with seconds), no explosion when the respective circuits are closed can take place, as there will be no continuity. If the explosion does take place, the enemy, being accurately in position to receive the effective shot of the water-cushion intervening between the mine and the ship's bottom, is either annihilated or fatally crippled.

The field-view through the telescope under such conditions is illustrated by reference to the following diagram:—



Every electric system of defence should be so constituted, that, should the enemy escape destruction by one explosion, he will be within reach of the area of destruction of the next series as he advances, each successive series being under the control of a properly organised system of observation and concurrent discharge from two stations.

The precise manner of the discharge and ignition of the mines does not fall within the scope of this paper. So varied are the apparatus and mechanical detail of manipulation that it is impossible here to enter into any technical description.

It is sufficient to call attention to the fact that the use of chemical detonating fuses, wherever the mines may be of long duration, should be avoided; time, temperature, and electric action greatly affecting the integrity of the chemical detonator.

Wherever the ignition of the mine can be effected by the heat evolved by the resistance of fine platinum wire, it is greatly to be preferred, as certain and secure in its action at all times, however long the mine may have remained in position. It is on record that the whole of the chemical fuses sent out for the defence of Richmond, but which from the fall of Wilmington and Mobile never reached their destination, were returned to this country, after lying a year at Havannah, and found to be utterly useless for the purposes for which they had been originally designed; not one fuse in one hundred fulfilling the essential condition of instantaneous ignition. [I place a few of these fuses on the table for inspection.]

Such being the result of past experience, it becomes a question whether, for any permanent system of national defence, any reliance can be placed upon chemical detonators. The exhaustive experiments made at Bowden near Manchester in 1863 by the late Lieut. M. F. Maury and the writer of this paper, with reference to the effect of the continued action of weak currents of electricity upon the sensitiveness of the chemical fuse, were conclusive against any reliance being placed upon that form of ignitor for any permanent system of electric defence. We can store our gunpowder without detriment from year to year. So also must our electrical defences be safe from deterioration, even though years intervene between the placing and the discharge of the mines.

DISCUSSION.

The CHAIRMAN: A very important subject has been brought before us, and one which I have no doubt will elicit a good discussion. I believe there are many gentlemen here who are conversant with the details of the subject, and I will, in the first instance, call upon Professor Abel to give us his views with regard especially to fuses and other points, with which he is well acquainted.

Professor ABEL said: I am sure we have all listened with great interest to the lucid manner in which Mr. Holmes has laid before us the conditions which should be fulfilled by an efficient system of torpedo defence. I think on most of those conditions those who have given attention to the subject will be agreed, and that others will agree with me that most of those conditions have been fully recognised for a number of years past in this country, in the patient endeavours of those to whom the task has been entrusted of working out an efficient system of torpedo defence. From the very first time that attention was directed to this subject seriously, viz., at the time of the American war, it was at once fully recognised that the application of electricity to the explosion of mines should form the foundation of any reliable system of defence, and on that fundamental principle the systems which have been worked out in this country have been based. At the same time, I think, and I believe others will think, who have studied the strong end, the points of electrical defence, that mechanical means must not be lost sight of altogether. There may be circumstances and conditions under which it may be very advantageous to apply reliable mechanical mines. There may be, for example, large tracts of water which it is almost impossible to command entirely by a system of electrical mines, and a portion of which may consist of shallow water, through which vessels of light draught may have access, and which it is possible to close entirely against large ships. In such instances, good reliable mechanical mines which can be laid down with safety, and which would remain efficient for a long period, would be invaluable; and I believe such mines, if they do not already exist,

will be perfected within a short period, and will add importantly to the value of any general system of torpedo defence that is adopted in this or other countries.

Mr. Holmes has not entered into the details of any particular system of defence, or electrical arrangements. I can quite understand his hesitation in so doing, for the subject, though a new one, is a vast one, and it is difficult, within the scope of a paper, to give a satisfactory account of the several systems which have already been very fairly tried. Mr. Holmes, however, has alluded to one portion of a particular system of electrical firing, and that is the employment of chemical fuses, as he called them, that is, fuses which are specially intended to be fired by currents of high tension. We have already had considerable experience in the application of these fuses to the explosion of both land and submarine mines; and there is no doubt a great amount of truth and justice in what Mr. Holmes says with regard to the liability to change of any chemical preparation—of any mixture of chemical powders, however permanent and stable they may be in their general character. This liability to change arises not so much from a want of stability in the materials themselves, as from the difficulty of excluding from a fuse as constructed till very recently the access of moisture; and access of a small portion of moisture to any mixture of ingredients such as an oxydizing agent, and with the oxydizable bodies of which the fuse is composed, must, in time, cause changes. I can quite understand that such fuses as these which Mr. Holmes has put on the table, and which were originally designed for the firing of guns for proof at Woolwich, and for experiments at Shoeburyness, would not stand for a great length of time exposure to a damp atmosphere. That liability to change has already been, if not entirely yet to a great extent, removed first of all by improvements in the construction of the fuse, whereby the access of moisture to the interior of the fuse is impeded if not prevented; and secondly, by an improvement in the composition of the chemical mixture used as the igniting agent in the fuse. At the same time, though, I would state my conviction that the system of igniting mines electrically, by means of fine wires, possesses many advantages; yet in some respects, in my opinion, it has not

the advantages of a system of firing by high tension fuse. It would be impossible to go into the comparative merits of the two systems. It would be beside the question to-night to do so, but I believe the ignition by means of wires is now being greatly improved, and that we shall before long have abundant proof as to which is really the most permanently reliable system of the two. I am quite sure, as regards the general question, the merits of this particular system of defence could not have been more fully or more ably put before us than has been done by Mr. Holmes this evening.

The CHAIRMAN: I should like to ask Professor Abel a question with regard to a system of torpedo which is both mechanical and electrical, which has not been mentioned in the paper or in Professor Abel's remarks. This was a system of combined mechanical and electrical torpedoes used in the Adriatic Sea during the Italian war. I should be glad to hear some observations on that combined system, with which, no doubt, Professor Abel is acquainted.

Professor ABEL: I believe no system of defence by torpedoes in which electricity is employed would ever be allowed to depend entirely upon arrangements for exploding mines by judgment. Such arrangements are excellent, but they should always be adopted in conjunction with other systems. The system in which, after all, very great, if not the greatest, reliance can be placed, is no doubt the system to which the Chairman has referred, in which mechanical appliances are employed as the means of establishing the electric currents, or completing the interrupted electric currents, in consequence of which operation the mine is exploded at the time a ship passes over it. The arrangement to which the Chairman refers is that of Baron Von Ebner, exhibited at the Paris Exhibition, and it was a very ingenious one for effecting this object. It was a mechanical arrangement so devised that the collision of the ship with the mine, or with the arrangements for firing the mine, determines its explosion at a considerable depth below the surface of the water.

The CHAIRMAN: The mine battery had to be put to the circuit, and then the mechanical contact would fire the mine; but neither process alone would fire it, neither the contact with the torpedo by

the ship, nor the connection with the battery by itself, would fire the mine, but both together would do so.

PROFESSOR ABEL: As far as I remember, one point which the Baron desired to avoid was the possibility of a fuse being in circuit with the electric cable till the moment it was to be exploded, and this was in order to avoid the accidental explosion of mines, especially in climates where there are great electrical disturbances, or explosions in consequence of the current induced in the cable by electrical disturbances, or the discharge of a neighbouring cable. The arrangement was so devised that the ship completed the firing circuit at the moment of striking, and thus brought the fuse into circuit, which was exploded. There are other arrangements of a comparatively simple character for accomplishing the same or similar ends, and the contrivance is known as "circuit closers" and "circuit breakers," and is an indisputable adjunct in the operations by submarine lines.

MR. HOLMES: I may be allowed to add, that, in speaking of mechanical means of firing as objectional in the torpedo system of defence, I had reference to cases in which torpedoes remain submerged for a long time. It is obvious that the action of sea water or even fresh water will cause incrustation or rust, which would render any mechanical means unreliable should the torpedo be submerged for several months. I referred to the case of the James River, where the mine had been submerged for thirteen months. There is no doubt that during warfare, when the submergence would be only for short periods, the introduction of mechanical mines would be advantageous, but for a permanent system of defence my remarks had reference simply to electrical mines, because if a mechanical mine is submerged for any length of time incrustations will take place which would impair its efficiency.

Captain MCEVOY (United States Navy) responding to the Chairman's invitation said, I have listened with a great deal of pleasure and much interest to the paper of Mr. Holmes, and also to the remarks of Professor Abel. The subject, as has been justly remarked, is a very large one, and one which will not allow a speaker on any short occasion to go into details in reference to it. I have myself had some practical experience in torpedo warfare, but that

was in the past; but the improvements have been so rapid and so radical that I think a reference to that period will scarcely be of any interest to the meeting. The improvements in electrical torpedoes are, I consider, quite in the right direction. I partly agree with Mr. Holmes, that mechanical torpedoes should not be entirely relied upon, and I also agree with Professor Abel that they should not be entirely dispensed with. For instance, I think, that, where a channel is defended by electric torpedoes, the flank should be protected by a mechanical torpedo of some sort, particularly when there is no chance of laying down a regular system of electrical torpedoes, where torpedoes would be of immense use. Mr. Holmes has given some instances with regard to the history of the operations against Richmond and Petersburgh, in which I personally participated to some extent, but those really amount to very little. He has paid only a just tribute to Captain Maury, who was one of the pioneers of the system of electrical torpedoes, and I think Mr. Holmes has done him no more than justice, and I believe the torpedo on the James River is the only one on record which destroyed a vessel in actual warfare. At Mobile there were no electrical torpedoes. At Wilmington a few were brought down, but were never used. I had charge of one torpedo, over which one of the largest iron-clads of the Federal Government lay for three hours, and all my efforts were made to explode it, but, from some defect in the wire, my efforts were in vain, owing, as it is believed, to the wire leading down from the shore into the water having been cut by the wheels of an ammunition waggon. This fact however was not known at the time, because the means of testing electrical torpedoes were then very imperfect. Had that feature of the system been as perfect as it is now, we should easily have ascertained the cause of the failure before the attack on Fort Sumter was begun. Had that torpedo been successful, 3,000 lbs. of gunpowder would have been exploded under the largest ironclad in the fleet, and the result of that you can only imagine. I saw the wreck of "The Commodore Jones" the next day, and I state that the account Mr. Holmes has given is quite correct as to the state of the vessel. I can add nothing more now, but, perhaps, on a future occasion I may be able to say something further on the subject.

The further discussion of the paper was adjourned to the next meeting.

The following candidates were balloted for and declared duly elected :—

As Foreign Members :—

Commandeur G. Pentasuglia	Florence.
Commandeur H. Pellegrino	Florence.
Chevalier C. Viale	Florence.

As a Member :—

Major P. H. Scratchley, R.E.	Woolwich.

As Associates :—

W. B. Clifford, M.A.	Harrow Road.
Arthur Fraser	Junior St. James's Club.
Oliver Heaviside	Great Northern Telegraph Company, Newcastle-on-Tyne.
W. Mays	North Woolwich.
W. Sparrow	Putney.
T. S. Tuffield	Woolwich.
E. M. Webb	Great George Street, S.W.
Herbert Waters, B.A.	Osnaburgh Street.

The Meeting then adjourned.

The Twenty-third Ordinary General Meeting was held on Wednesday, the 25th February, Mr. LATIMER CLARK, Vice-President, in the Chair.

The following communication from Don Ramon Pias was read:—

(Translation.)

Geo. E. Preece, Esq. Santiago, 22 December, 1873.

My dear Sir,—I have had the satisfaction of receiving your letter of 13 October. From it I learn that the Society of Telegraph Engineers of London have honoured me with the title of Local Honorary Secretary of the said Society. I accept so honourable a distinction, and I will co-operate, as far as my part is concerned, to duly fulfil my commission.

In a short time I will send a detailed account of the existing telegraph lines in Chili, the materials employed upon them, the system of apparatus, batteries, &c. adopted.

At present I will allow myself to make a few humble remarks on the subject of the insulators employed both in Europe and Chili.

Generally they are of zinc, porcelain, glass, or glazed earthenware.

The first are very unsuitable, especially for the American Republics, owing to the heavy cost of freight, and they are moreover too heavy for placing on wooden poles and for the linemen to carry with them to replace those which become defective. They are, besides, badly insulated.

Those of porcelain, glass, or earthenware are very weak, and the composition employed to insulate them and fasten the iron hook comes out from the cup without the latter being better insulated than the former.

In my opinion the insulator should be of thin plates of wrought iron with the inside tube of porcelain or glass, or perhaps of ordi-

nary earthenware. This tube should be 4 to 5 inches long and 2 in circumference.

The hook for supporting the wire should be 2 inches from the extremity of the cup, and terminate in two small hooks in which the wire may be twisted, which would obviate the use of tension insulators (winding insulators), for all the insulators in this form would serve for tension by crossing the wire upon them, which would be that known as No. 8 or 9. The idea is not mine; I take it from a sample which has been sent me from the United States, which has the defect of being in cast-iron and of an extraordinary weight; if made with wrought-iron this great defect would be avoided.

I would conclude these remarks by condemning all insulators which are fastened to the poles with screws.

The model which I send to the Society has a large screw joined to the insulator 8 or 10 inches long, so that its fixing is not only more easy but more solid, and separates the insulator 2 and 4 inches from the pole, so as to allow 4 or more lines to be placed on each side of the pole at convenient distances apart.

Would that gentlemen so able as those who form the Society might resolve this question in the sense of the economy and solidity of telegraph lines. If such an important improvement were attained the telegraph lines of the new world would multiply a hundred fold.

Accept my respects, and be good enough to convey the expression of my thanks to the Council and President of the Society of Engineers for the distinction conferred on

Your faithful servant,

RAMON PIAS.

The following paper was then read:—

MILITARY TORPEDO DEFENCES.

By Nathaniel J. Holmes.

The carrying out of an electrical system of torpedo mines for land defences against the attack or approach of an enemy is one of much greater difficulty than the protection of a coast by simiiar means by sea. Instead of the facilities which present themselves in the submarine defence, by the easy submergence of the wires and mines, and the silence of the trackless water, which leaves no trace behind to point out to the enemy the position of the sunken mine, new elements come into play when dealing with earthworks, and superincumbent masses of a yielding and elastic nature, that require specially devised plans and a distinct system of operation. Although with submarine defences wires may sometimes be submerged in the teeth of the enemy under cover of darkness, it is not possible successfully to mine and dig trenches in the face of an advancing foe. Traces of such operations, however carefully carried out, are sure to be discovered by the enemy, who could then easily destroy the defence by cutting the wires could any clue be obtained to their position. In the Crimean War of 1854 the Russians appear to have devised some kind of electrical mines for the defence of the Malakoff Tower, as in digging the trenches for the assault of that position insulated wires were found and cut by the English officers, and it is well known that Professor Jacobi at that time introduced the use of mechanical torpedoes as a part of the Russian defence in the Baltic Sea against the approach of the English Fleet. Fortunately these torpedoes proved to be utterly powerless to destroy any of the vessels which came in collision with them. So with the mines laid down at the Malakoff, no effective results followed. At Richmond, in the American Civil War in 1864, mines were laid for the defence of the approaches of that city, but never sprung; and in 1871, while Paris was in the hands of the Commune, electrical mines were in part carried out in some of the subways under the city, but fortunately for Paris removed by the Government troops before any destructive effects were experienced. With the exception, therefore, of these one or

two inoperative examples, there exists no precedent to guide the military engineer in the construction of his defence.

With sea mines in naval warfare it is possible in twenty-four hours to establish a defence. With military torpedo defences this is not possible; every position occupied, and every point of attack laid down, requires to be covered either by artillery or adjacent mines, and therefore to form a part of a concerted system. Thus any efficient electrical land defence should, if possible, be established before an enemy is in the field, each successive line and system of "advance" mines trenched out, and the insulated conductors brought into their respective centres of operation; the several stations for concurrent angular observation being settled and the mines placed in readiness for effective discharge, so that at any moment in case of war they might be called into action.

When therefore an electric system is to be employed in military tactics as an auxiliary to that of the defence by heavy ordnance, the position of the circuits, and the area of destruction of the mines, must of necessity be concealed from the enemy, and the successful carrying out of these defences by a secret service department will in a great measure frustrate the designs of spies and informers giving information to the enemy of the position of the mines and wires. Originally it was intended that the chief approaches to Paris in the war of 1870 should have been covered by means of a series of subterranean electric mines, and a very efficient system had been prepared, and would have been carried out, by which all approaches into the city would have been effectually closed, except at a vast destruction of life. With the intended system it would have been unnecessary to have dug the successive pitfalls along the avenues leading to the Arc de l'Etoile. No army could have passed the deadly series of fougades placed in position.

To the official incapacity of the several administrative heads of the Government departments in office, at that critical moment for the Empire, the non-completion of this defence may be attributed; the advancing Prussian lines were almost within gunshot before any serious consideration was given to the subject, and, with their flying columns and scouts over the ground, all such operations

then became impracticable. It is here perhaps interesting to explain the nature of the then intended defences to the approaches of Paris. Time would not have admitted of any such special system of defence as the writer of this paper would suggest should be carried into effect for every system of permanent defence where electric mines are to be employed as an auxiliary to the range of artillery, but still sufficient time remained to have made every approach around Paris impregnable to the advance of the Prussian army, by the placing of mines without the lines of fortifications under cover of the guns of the several forts, and other mines within the line of fortifications, controlled by observing-stations from the top of some of the more prominent public buildings. By this means a treble line of effective defence would have been established:—

First. That at a distance beyond the range of projectiles from the forts, and which would have operated against the establishment of the Prussian batteries upon the heights surrounding Paris, and the firing from which caused so much destruction to private property.

Second. The defence afforded by the artillery of the several forts.

Third. That of the secret service fougade within the lines of fortification closing the approaches into Paris. One or two well-directed "shots" would have terminated the march of the most enthusiastic general, ignorant as he would be of the nature of this internal defence. It is fearful to contemplate the amount of death and destruction that would have taken place had these mines been laid and sprung grape shot. Segment shell and the mitrailleuse in their destructive effects could not be compared for a moment with the fearful Aceldama they would have caused. These land-mines, concealed beneath the surface of the ground, and constructed in the form of a shallow inverted cone, into the apex of which the bursting charge is placed, calculated to "throw" the superincumbent mass composed of some hundred tons of broken granite and paving stones, would be about as deadly a fougade as could well be conceived; on its explosion death would be sown broad-cast, and the successive discharge of two or three such "detonators" would almost annihilate the advancing columns of an army.

As it was, the indecision of the French Governmental heads of departments deferred the consideration of this all-important system of defence until it was too late to be made available. The Prussian Army, fast advancing upon Paris, and gaining position after position, frustrated any organised system of long-range defence; while at the same time, the Government of Great Britain, having declared that telegraph stores and coals were contraband of war, effectually prevented the proposed defensive measures being carried into effect. The value of electrical mines for military defence is greatly enhanced when the same are laid down as a permanent system, auxiliary to that of forts and earth-works, for the maintenance of any special fortified position, because it is then quite easy to extend the wires without detection to distances covering the approach, beyond the range of guns. Properly trenched into the earth at depths not less than from eight to ten feet, the wires would be as secure from injury as most sub-marine cables lying in deep water. The chambers to contain the charge should likewise be constructed in a permanent manner. At a comparatively small expenditure, masonry might be employed in the construction of watertight compartments, wherein the charge could be deposited for any period without detriment to its explosive properties. If a charge can be submerged for thirteen months under water, as was the case with the James River mine, a military defence properly laid could be maintained for an indefinite period under ground.

In military defences, by means of the electric agency, the employment of mechanical "contact breakers" and "circuit closers" is inadmissable. Circumstances might possibly arise in submarine defences which would admit of such arrangements being introduced, but in military torpedo works they are absolutely inadmissable. It may here be desirable to make a few remarks upon the employment of "mechanical torpedoes" and "circuit closers" in action. First, mechanical action of all kinds, however simple, is unreliable; because, however perfect the apparatus may be in its construction when first placed in position, variations of temperature, rust, corrosion, seawater growths, friction, and the many uncertainties incident to all mechanical appliances left to

themselves for any length of time, destroy all reliance upon their accuracy of performance. Now, in warfare mechanical torpedoes once placed are left to themselves; to inspect them, or overhaul the working of their parts, is fatal in two ways, possibly to the forlorn-hope sent to carry out the inspection; and secondly, because any such inspection might give the enemy, always on the alert, intimation of their existence and a knowledge of their position. Reliance, therefore, upon any such mechanical apparatus may prove most disastrous to the defence—when most required. As in the case of the Richmond fuse-mine, they are in nine cases out of ten inoperative.

Again, mechanical appliances depend for effective explosion not only upon the perfection of their mechanism but upon percussion from actual contact with the enemy. This limits the field of action, as unless the enemy actually strikes the mine no explosion can take place; but as the effective area of destruction of a properly constructed mine is equivalent to that of a circle, the diameter of which is 90 feet, with the electrical system a ship may be fatally injured even though not immediately over the mine, so long as she is within the area of its destructive effects.

In the defence of Venice against the attack of the French in 1859, when the Austrians under Ebner fortified the sea approaches to Venice by means of electric torpedoes, it is interesting to recur to the ingenious method they adopted to insure the explosion of the mine only when the enemy was within the area of that special defence.

By the employment of the camera obscura the position of the various mines as laid down by the Engineers was marked on the obscura chart, and the area of their destructive effects indicated on the map by a circle, so that the approach of any of the enemy's ships into the Bay of Venice would have been observed by reference to the obscura chart, and, as soon as they passed within any of these destructive areas, electric currents were so arranged as to explode the mine and discomfit the advancing foe.

The armistice that followed prevented the completion and testing of these proposed defences under the ingenious system at that time devised by Baron Ebner, and the few that the Austrian Govern-

ment actually laid down were removed without having been called into action. This defence however marks an epoch in the advance of torpedo engineering. The mechanical torpedoes employed by the Russians in the Baltic Sea in 1854, and devised by Jacobi, consisted of hollow conical vessels charged with powder, the ignition of which was dependent upon the act of concussion with the enemy—or, as it might happen, friendly ship—driving home a bar arranged to ignite in the interior of the torpedo case a chemical composition placed in the immediate vicinity of the charge, and so explode the mine. It will be easily understood that, had these mechanical mines remained for any length of time in the water, incrustations and rust would soon have rendered them inoperative. The few that did explode proved the arrangement to be only a mechanical toy. It is only within the last few weeks that a fatal accident is recorded in the Government laboratory with one of the mechanical fish-torpedo self-propelling missiles now being manufactured and experimented upon by the Government departments in smooth water.

How far these mechanical self-propellers may be found available in actual warfare is as yet undecided, and may well be a matter for grave consideration, while the value of the simple electrical system of defence by land and sea is at present more or less neglected.

Built up to form an integral portion of our forts and harbour approaches, our fortifications, trenches, and breastwork enceinte, how valuable would this system prove did the stern realities of actual warfare arise. If our naval and military administrators would consider two points: First, that the employment of the electrical agency as a direct means for exploding mines by land and sea is simple, inexpensive, and absolutely certain, while at the same time safe for manipulation, transport, and accuracy of fire; and, Secondly, that the introduction of mechanical apparatus to take the place of electrical agency is more or less uncertain, costly, and dangerous to life in the manufacture, transport, testing, and planting of the mine, there would be every reason to believe that ere long permanent electrical forts and fougades would be established as an integral part of the system of our national defences, and the introduction and use of mechanical means for the ignition of

the charge, or in the attempted mechanical propulsion of a detonating mine in a seaway, would be very cautiously entertained.

In concluding the subject under consideration in this and the previous paper, the intention has been to point out and bring before the members of this Society, not so much a discussion or advocacy of any special system of manipulation, or any elaboration of practical details of apparatus, circuits, and mines, so much as to point out that the true value of the electric torpedo defence consists in the employment of the direct action of the current as alone affording that security without which no torpedo system can be reliable. Of the importance now attached to the electric torpedo system of defence, the torpedo armaments of Russia, Sweden, Norway, and the United States of America testify. It is a matter for reflection to learn that while in 1865 the Governments of France, Holland, Belgium, Denmark, Norway, Sweden, and Russia paid special attention to this subject and recognised the value of the defence, at that time the Government of England remained apathetic, and it is a question how far our hybrid system of mechanical ignitors and circuit-closers and compressed-air propelling mechanics can in action compare with the splendid organisation of electric torpedoes that now form an integral part of the naval and military defences of Russia, the United States, and Sweden.

DISCUSSION.

The Chairman: Mr. Holmes may perhaps now favour us with a few observations on the paper he has just read, and also on the paper which he read at the last meeting, and which has been referred to. We are all electrical people here, and I dare say we shall all readily agree with Mr. Holmes in admiring electrical means of ignition. I fear, therefore, the discussion may not be such an interesting one as it would have been if Mr. Holmes had described some plan or system of his own. I do not know whether I am rightly informed. I believe Mr. Holmes has some peculiar form of torpedo which he wishes to recommend. If he has I think it would be very gratifying to the meeting if he could tell us the

nature and kind of wire, what he recommends as the means of ignition or arrangement for firing, and, in fact, generally describe the system which he would recommend.

Mr. Holmes: I was not prepared, gentlemen, to advocate any particular system. What the Chairman has stated is quite correct. At the time of the American war a special system was devised by the late Captain Maury and myself with a view to give greater security and efficiency in the discharge of electrical defences. There is now so much known—not only that system is known to everybody, but it has been published and it has been investigated by four of the Governments of Europe. I have given the names of the different Governments who at that time sent over special officers to receive instructions on the different modes of defence that had been adopted by Captain Maury. That particular system is, therefore, well known. Although from that time to this the subject has occupied a great deal of my attention, I am not aware that I have any very special remarks to make in addition to the plan that at that time I enunciated. The experiments that have been carried out by our own Government will most probably have elucidated new facts. Of course, whatever facts are known, one of the great values of torpedo defence is secrecy. The Americans keep their affairs in perfect secrecy; so do the Russians and the other nations who adopt the system. It would not be wise if our Government disclosed any of their special arrangements; and I do not propose myself to enter into any details on the subject unless any special questions might be put to me which I might think it desirable to answer.

The Chairman: We shall be glad now to hear a discussion on the paper if any gentleman has any observations to make.

Major Malcolm, R.E.: Mr. Chairman, this is an entirely new position for me to be in. Mr. Holmes has evidently taken a great deal of trouble to get information from other governments, talking in the first instance as if we had done nothing; and I was not prepared for a discussion of this nature; neither was I prepared to see on the table instruments which have been elaborated under my own supervision and the supervision of my predecessor, with the assistance of some gentlemen who are at the present moment

in the room, and which the War Office and Admiralty flatter themselves are profound secrets. I suppose the seal of secrecy may be taken off me too, and I may say for the information of those who do not know me personally that we have a school down at Chatham which is called the School of Submarine Mining, of which I am at present in charge, and we have gone, as closely as time and various opportunities admit, into the subject of electrical firing, mechanical firing being almost entirely put on one side, and not contemplated, except for special services, on account of what we believe to be the almost insuperable difficulties of putting the torpedoes down, and, worse still, of taking them up after the necessity for them has ceased.

I was not able to come up a fortnight ago to hear Mr. Holmes, but I have taken the liberty of making a few rough remarks on his paper, which I suppose has been in the hands of most of you. I really do not exactly know what to say. The Government gets attacked very often in this kind of way; but when we come to examine the attack—I hardly like to call it an "attack" either, but when we come to examine the papers read or communicated to the press—we find a terrible quantity of, what seems to men acquainted a little with the subject to be, mere vague talk. Now, I will take the second page. I find, "as mechanical appliances can be obtained at less cost than the maintenance of a standing reserve, the defence of a kingdom resolves itself in great part into the establishment of properly applied scientific systems, controlled by intelligent operators, as against the too often lavish expenditure of revenue in the construction of unwieldy armaments, of guns, and armour-plated ships competing for mastery," and so on. Very fine; torpedoes are all very well in their way; but an island like ours, with numerous colonies and vast trade, must have ships and massive guns. You may put down as many torpedoes as you like: you want them to stop where you put them down, because if they do not your camera obscuras or anything else will be of very little use; but you must have ships to protect your commerce, and guns, alas, as well. Then, almost in the next line, I find, "With the electric torpedo this competition ceases. Simple in its construction and action, called upon to expend its power upon the enemy without reciprocal challenge, the

most powerful Monitor or armour-plated ship ever constructed easily falls a prey to its deadly embrace. At the cost of a few thousand pounds the strongest ship with its heavy guns and gallant crew, fitted out at a cost of hundreds of thousands of pounds, becomes comparatively inoperative for the attack." But if a torpedo is to cost a few thousand pounds it will puzzle anybody to make it very simple, unless you make it of gold or platinum or something of that kind, which seems to me to be wrong.

Then again, "Half-a-dozen men in control of the torpedo mines can effectually keep at bay both an army and a fleet." Now, what can half-a-dozen men do? Where can they be? You talk in other parts of the torpedo getting wrong, and wanting to be overhauled, and so on; how are half-a-dozen men to do it? And when you come to go into the matter you find that a great many half-dozens of men are wanted; and yet it seems in reading a paper like this that it is so exceedingly simple. I assure you it is exceedingly difficult. Then we have the James River torpedo. That is quoted: that was an electric one. It is said that the defence of the river was entrusted to a single electric mine. I speak now without book, but it is my impression that there were a great many mines put down and a great many mines taken up, and there happened to be one which was not taken up, and which went off at the right time and did its work; and I think, also, if I have been rightly informed, that there were a great many more ships than one destroyed in the American War, and a very large proportion of them was destroyed by mechanical mines. Nevertheless, we do not use mechanical mines; at least we do not contemplate the use of mechanical mines in this country to any great extent, on account of the terrible loss of life to which they expose the people who work them.

A case is quoted in the paper as to the Prussians. They are said to have lost a great number of their own men in that way. Then, again, here is a curious vagueness, and as you are electricians I do not mind taking you all with me in this: "To render any system of electric torpedo defence practically useful and reliable, it must be under the perfect control of the officers and men using that system." That you will thoroughly understand. Well, how are

you going to make it so that any inexperienced officer is to work it when it is going to blow up? It cannot be done. There are two or three little difficulties that occur to me; and, if any electrician here will solve the question for me, I shall be only too glad: I shall only be too delighted to take the hint. One difficulty is this: it is stated, "It is likewise necessary that the electrical arrangements shall ensure the ignition of the torpedoes by land and sea at distances exceeding the effective range of cannon," which is, of course, very easy. It is just as easy to fire a mine whether it is three or four or forty miles off if you have only enough battery-power. "And that, when necessary, more than one mine shall be exploded in group simultaneously on the same circuit." And then, again, you are to have the power of discharging mines, which I suppose is exploding mines, even if the enemy should succeed in breaking your wires. I confess that this is more than I know how to manage.

Then the power of testing mines at all times has been most carefully thought out to the best of the abilities of a good many officers who are daily working at that; and it is not our opinion as a rule that it is at all desirable to go talking through a mine. We can generally manage to communicate in some other way if we want to. Then here is a very interesting thing on record about M. Chabannes at Toulon. They were knocked down on the jetty, and there was no sign on the surface of the water. Now, I should have been very glad to come up a dozen times from Chatham, as I have come up to-night, in order to get a little more information upon that,—if only the writer of the paper had told us what the charge was, and what the depth was, and how far off it was; but papers that leave out all these things are, for our purposes who are charged with the torpedo defences of the country, absolutely useless.

As for the chemical fuses and the platinum wire fuses, it may be interesting to you to know that we use both: we have them both. I have read of a new ship out in America—I believe it was called the "New Ironsides"—that was for two hours grounded on an electrical torpedo, but it would not go off, and they very nearly shot the officer in charge as a traitor in consequence. So you see

you must make some improvements upon what they did in those days. They did very well; and the Russians did very well considering.

Professor Jacobi had not only a mechanical system, but an electrical system; but they had an exaggerated idea of the power of powder, or too mean an idea of the strength of the ships, I do not know which; but an officer who picked up a good many of the torpedoes told me that they put in from 10lbs. to 15lbs. of powder, and if any of them exploded at all—which many of them did—they went off like squibs and did not hurt any one. Again, the author says that in twenty-four hours a harbour may be made safe with sea-mines. Are the six men to do it? because I can tell you they won't get many mines down. Then the system that the author proposes for his ideal military mining is—what shall I say?—very difficult to carry out.

Doubtless you are aware that there is a corps that used to be called "Sappers and Miners"—(they are all called Engineers now)—who have something to do with mining on land. Round every first class fort there is a system of what are called countermines prepared, and they have been prepared ever since the beginning of the sixteenth century. These mines now-a-days would undoubtedly be fired by electricity as they are constantly fired at Chatham, and have been—(and thousands of people have admired them)—for the last twenty years—I can remember twenty years back. Sir Charles Pasley, R.E., had a good deal to do with electrical firing so far back as when destroying the wreck of the Royal George—over thirty years ago. But war is a question of money after all, and this grand idea of sowing mines broadcast all along the roads to Paris, and frightening everybody by shooting them off from time to time, is greatly a question of money; and I should like to ask how far they are to extend, and how many mines you would have, and what battery power you would have, and how you are to manage to keep them all secret when you have got them down, and how you will manage to be near enough to know when you have a flank of enemy near.

And I should also like to say, that when we advance in civilized warfare where there are roads and fields, in Ashantee it is not quite

so simple, and they unfortunately are compelled to keep very much upon the roads they cut; but in Europe at any rate you need not go along roads, and the usual way is not to advance only along roads. No army would think of advancing along a road until it had sent a number of men first. All the roads and fields would be carefully examined by ones and twos and threes in skirmishing order, who would soon find out your preparations, and I think you would hardly get off more than one or at most two mines, so that you would find that your plan had wasted a great amount of money. If, on the contrary, you had a fortress, which nobody in England will allow you to make—though we have one or two in India—a regular fortress with an *enceinte* of 2,000 yards round it, in which no roads are allowed to be cut, you can have such a system; and if you have an army close to you, you could pop them off now and then, but you would do no damage when you had done so. You might kill some few men, but men come into the army to be killed. The old-fashioned way is to reserve this sort of thing until you have got the men pretty close to you, and till you can rely upon making every mine do work.

I hope you won't think I am warm in this matter; but official incapacity is constantly brought forward when great inventions fail, because they are no good, as if the departments of the Government had nothing to do but to sit upon inventors and make themselves generally obnoxious. I am sure they do not want to. I am sure that they try very hard to use for the good of their country all practical suggestions made to them. Of course they have not infinite knowledge; but who has? Then about the unfortunate Whitehead fish-torpedo that exploded the other day. It did not do anything more than hundreds of boilers do every year. That I happen to know. I saw the torpedoes that were brought over some time ago by Mr. Whitehead at work, and deliberately I say that I think it a very valuable invention. And one final remark I would like to make about Russia, Prussia, Austria, America, and all the rest of them. Austria was not a very wealthy country when it paid to Mr. Whitehead, I think, £15,000 or more for the secret of the fish-torpedo. And a very short time ago anybody might have seen in the Estimates the

torpedo defences of this country put at something like £200,000; so that you see that the Government has been alive to the advantage of submarine warfare, and, if you will accept it from me, the system of electrical ignition has been recognised and adopted long ago both by the army and navy.

Professor ABEL: I may, perhaps, as a matter of interest just refer to one or two points that Major Malcolm has spoken of connected with the application of electricity to the explosion of mines both land and submarine. He has rightly said that Sir Charles Pasley was the first to apply electricity to the explosion of mines, rather more than a quarter of a century ago, in England, some few years before experiments in the same direction had been made in France. Those were the first experiments that were at all made in connection with the explosion of mines by electricity, and Sir Charles Wheatstone and Professor Daniell of King's College were very naturally much interested in the application of electricity in this direction. Colonel Pasley was acquainted with these gentlemen, and it was in the laboratory of King's College, about a quarter of a century ago, that the first arrangements for the explosion of submarine charges were worked out. They were applied to the blowing-up of the wreck of the "Royal George" at Spithead, and, not long after that, electricity was successfully employed for the explosion of land-mines by the engineers especially under Colonel Pasley's direction.

In 1854 one of the most interesting, and I may say one of the most elaborate, papers that have ever been published up to this day in connection with the subject was published by Captain Ward in a very valuable series of papers not generally known—"The Professional Memoirs of the Royal Engineers." In that paper he went very much into detail with regard to the batteries that should be employed, and with regard to the description of fuses and conducting wires which he had selected by careful calculations and verified by actual experiment. He showed the amount of battery power that should be employed for particular lengths of wire, and went very much into all the details which have been discussed over again since. One result of his labours was that the improved Grove battery was introduced into the Royal Engineers service and is still in use.

Then in 1856 Professor Wheatstone and I happened to be members of one of these very much abused Government Committees. I am happy that an outsider on this occasion was connected with it, Professor Wheatstone. He it was who proposed that the subject of the application of the electricity of induction and the electricity of high tension of different kinds to the explosion of mines should be carefully inquired into, and it was in consequence of that that he and I carried on for some years, both at Woolwich and Chatham, a series of experiments which resulted in the development of the magneto-electric explosion machine of Wheatstone and others, and also in the production of improved high-tension fuses. About the same time, or a little before, Baron Von Ebner in Austria had applied frictional electricity successfully to the explosion of mines. I may say that as far back as Franklin and Morse frictional electricity had been so used in a somewhat uncertain manner, but it was Baron Von Ebner who first worked out the system of doing it.

Then a Spanish officer and also a French officer in conjunction applied the induction coil at the same time, and so several, not only in Austria but also in France and England, were working together at the subject and applying electricity from different sources to the explosion of mines, and I do not believe that the subject has for a single month, in fact during the last five and twenty years, in this country, been neglected. I am sure and perfectly confident that there is no country that is really in advance of us in this respect, and in confirmation of that I may refer to the fact that we have received continual visits from foreign officers with special credentials referring to us to receive information, which we are most willing and happy to give, with reference to the arrangements that we have adopted and elaborated for the explosion of mines both land and submarine.

With reference to the special points touched upon in Mr. Holmes's paper, there is only one that struck me as suggesting a question. I should have liked to ask it on the last occasion but I did not. He deprecates the use of mechanical elements, and I presume that by that he means not merely purely mechanical mines, but also as he told us this evening circuit-closers, except in special instances. Now, I cannot conceive how any system of electric mines can be perfect

which shall be harmless at night, or at any rate in weather which is not perhaps foggy, but not sufficiently clear to allow your working by means of the telescope. I cannot see how any system can be efficient without being supplemented by a circuit-closer and breaker. The two systems must be combined. It is impossible to separate them. It is possible, perhaps, to use the circuit-closer alone, but it is impossible in any system of submarine defence to use firing by observation, leaving out a circuit-closing and circuit-breaking system.

Mr. TREUENFELD: There is one remark in Mr. Holmes's paper which was read at the last meeting. He says: "In torpedo warfare we have so far no precedent but that of the American Civil War of 1864." I beg to be allowed to draw the attention of Mr. Holmes to the fact that there has been another torpedo war of perhaps equal importance and of longer duration than that in the United States in 1864, a war which during four years was permanently operated with torpedoes. The war lasted six years altogether, and torpedoes managed to keep back a navy of more than fifteen ironclads and fifty or sixty men-of-war during a period of four years. I allude to the war between the Republic of Paraguay on the one side, and the Republic of Brazil, the Argentine Republic, and the Republic of Uruguay on the other side. The Republic of Paraguay was blockaded during six years by the Brazilian fleet, consisting at times, as I say, of as many as sixty man-of-war vessels and fifteen ironclads, and by the Brazilian army, and the army of the three countries, being composed sometimes of as many as 80,000 men.

I was several times in charge of the torpedo department in the Republic of Paraguay. We used chiefly mechanical torpedoes, as we had no materials in the country for anything else, and we had to manufacture everything ourselves. That was the reason we chiefly used mechanical torpedoes, and by means of these mechanical torpedoes this immense fleet, combined with an army of from 70,000 to 100,000 men, was for four years kept from making any rapid advance. We also tried to use electrical torpedoes, but as we were blockaded and could obtain no materials for the purpose we had to make the cables ourselves. My assistant engineer, Hans

Fischer, who died in the war, succeeded in manufacturing cables. But we had great difficulties in the manufacture, as we had to send the soldiers out to tap the india-rubber trees and draw the milk, and we could not produce as many cables as we wanted. I mention these facts as I believe they have never been printed, and may be very little known amongst torpedo engineers. In the Republic of Paraguay we had at least 300 torpedoes laid down. This historical fact may, perhaps, speak in favour of mechanical torpedoes. Anyhow, they are very useful under such circumstances where other material is not at hand. They proved to be of very great importance in this war, and, in fact, they were perhaps the reason why such a war could last for six years. If it was not for the torpedoes it ought to have been finished in one year.

The CHAIRMAN: Will you kindly describe the nature of the mechanical torpedoes which were used.

Mr. TREUENFELD: The torpedoes were moored, and they had chemical fuses in glasses which broke when struck. Sometimes there were bars coming out, and these when struck by a ship broke glass bottles which were inside. The electric torpedoes were simply torpedoes with platinum wire.

Professor ABEL: Had you any casualties in laying them down and taking them up.

Mr. TREUENFELD: I am sorry to say that I am the only one who had anything to do with the torpedoes and who still survives. Those who did not blow themselves up died in the war.

Lieutenant SCOTT, R.E.: I am under the great disadvantage, Sir, of not having heard the paper, but I gather from Professor Abel's remarks that the author prefers a mine without a circuit-closer, and which is fired by observation. Now I may say from experience that in the rapid tideways often found at the mouths of harbours it is not only difficult but impossible, unless moorings are laid down beforehand, to insure putting a submarine mine in the position you wish to; and, if you lay down your moorings beforehand, then where is your secrecy? I think secrecy is an important point. Professor Abel has already spoken of the cases of night and fog, and so forth, in which you cannot possibly see, but I just wish to

say from practical experience that it is as a rule impossible to place a torpedo at the bottom of the water, with the absolute accuracy required for observation-firing, unless you have moorings previously laid down.

Lieutenant WATSON, R.E.: I should like to ask Mr. Holmes one question with regard to his first paper. He said that the area over which a torpedo would produce a destructive effect on a ship was 90 feet; but he did not tell us what the depth of water was, what the charge was, or what sized ship he was speaking of. It is known from experience that those three points make a great difference, and that a torpedo which has a certain charge will have a very different radius of effect from one which has a larger charge. I ask Mr. Holmes what charge he was referring to, and what experiment he based the observation upon? I came here to-night to get some information on the subject. We have had a great many experiments upon the subject, and it is of very great interest to us. I thought that, perhaps, he might be able to explain more fully upon what he founds his observation.

Mr. ARTHUR R. GRANVILLE: I would like to ask what effect earth-currents would have upon torpedoes, because if earth-currents had any effect upon the fusing of the platinum wire, or any other electrical means of firing, it seems that they would do away the great efficiency of the permanent torpedoes.

Mr. HOLMES: I would make one or two observations with regard to the remarks which Major Malcolm has offered. He referred to the paragraph in my paper which states that at the expense of a few thousand pounds a defence could be successfully maintained. It is quite unnecessary to construct your torpedoes of gold and silver, according to the remarks he made, in order to expend that amount. The expense of an electrical defence is not in the mine or in the fuses for firing the mine, or in the apparatus employed to produce the current to fire the mine. The expense of torpedo defences is in the proper maintenance of the insulated electrical circuits. When, therefore, I use the expression "at the expense of a few thousand pounds," I am referring to a properly laid down and organized defence, in which the electric circuits themselves take a chief part in the expense, they being the most

necessary part of any permanent defence. In the same way, when I say that a handful of men, five or six men, with a properly constructed defence, can successfully defend a position, I am not speaking of a defence laid down at an hour's notice. I am speaking in the paper that I read to you of an organized defence, a permanent system of defence; and I laid that down as the rule before I commenced any illustration or description. I refer to this diagram, which will show that a handful of men, four men, could defend the whole of Portsmouth Harbour. Each of these blue lines and red lines represents an electrical circuit. The red spots represent mines. I have simply illustrated three mines, but you might have thirty or you might have 300. The concurrent observation of three men from each of these forts would give them the command of 300 mines as simply as three. It does not require a large body of men to control a well-constituted system of electrical defence. Such a defence, properly laid down and accurately planned out, can be maintained at a very small expense; and I contrasted the unnecessary outlay of money in heavy guns and ironclads, because we all know that the expense and calibre of our guns has been increased according to the impenetrability of the plates with which our ships are coated. The plates are made heavier and thicker as the guns are able to penetrate. There is no end to that war. The heavier the plate the heavier the gun. With the torpedo, I state, this competition ceases. A torpedo properly planned will act effectually on the heaviest armour-plated ship as well as on a wooden ship. It is a question for experience to decide whether a heavy armour-plated ship is not of more easy destruction than a wooden ship. I believe she is, and I believe that the experience of the James River mine tends very much to prove that. Then again with regard to my speaking of the area of destruction of a well-planned system as a circle whose radius would be about 45 feet. That statement is derived from the statements made by the Americans, and by Captain Maury, who is now deceased. I have his experiments in his own handwriting, and those are all dated. They are what he and I worked together.

Lieutenant WATSON, R.E.: May I ask what the charge was?

Mr. HOLMES: When I make that assertion, I particularly state

in my paper that I cannot go into any details, but the charge of powder has to be regulated by the cushion of water. The words I use in my paper are: "So that the effective shot of the cushion of water shall penetrate to the bottom of the vessel." Water being nearly incompressible, the effect of the explosion of the mine is to drive the column of water to the bottom of the ship. The column of water takes the place of a shot, and I guard my expression by stating that the charge of powder has to be regulated to the depth of the water and the area of destruction. In respect to Admiral Chabannes I have seen his letters, but I believe that the depth of water was sixty fathoms, and the charge thirty pounds. He wrote a very long letter to Captain Maury and myself upon the peculiar results of his experiments.

I very much regret that Europe has not had the advantage of the most interesting narrative of the torpedo defences in South America. If it were possible, I should like a little more information to be made public on really so important a defence than is known at present. I confess that I was in perfect ignorance that it had assumed that proportion. I was aware that torpedoes had been used, but I did not suppose for a moment it was to the extent that I have been informed this evening. The results of those mechanical torpedoes has been most important, but still we must bear in mind the nature of the defence which took place in South America. It was a river defence and easy of access. It is not like the open sea, or like a harbour approach, where you have a vast surface to cover. You have there a channel not a tideway, but a current always going in one direction flowing to the sea, and therefore there are certain facilities connected with a mechanical defence of that kind in South America which do not always exist in open seas.

Professor Abel: I was very anxious to hear from Mr. Holmes how he defends a passage at night by torpedoes without any self-acting arrangements.

Mr. Holmes: I think you are aware that the plan proposed by Maury was to defend his torpedo line more against the attempt of the enemy to drag for them. That was by covering the line of defence or "raking" it with guns, or by throwing an electric

light or lime light where the enemy would have to pass, so as to see the position; because in torpedo systems it is not to be presumed for a moment that you are to work in the open sea. You are generally either in a harbour, or in a channel, or in some channel-way where you have not got such a very large expanse to attack. That is, in the case of the torpedo defence of a sea-coast. I confine my observations more especially to the water defences of harbour and channel, where you have, or may have, such facilities. Of course, the same objection that I have made against all mechanical appliances, even though circuit-closers are brought into action to defend at night, still holds. You may rely upon the circuit-closer acting, and it may fail to act. You will place your dependence upon it, and other means may be neglected. Then a vessel passes the circuit-closer, or strikes the circuit-closer, and from some mechanical defect the vessel escapes. I object in my paper to mechanical apparatus, simply because mechanical apparatus will never, for two weeks together, be reliable unless it is continually watched; and I maintain that you cannot, when you plant mechanical apparatus, be continually going in boats to inspect it. The great point in a system of torpedo defence is that no intimation shall exist in the face of the enemy that anything is laid down.

Professor Abel: But is not it better to have two strings to your bow, and also to have a chance of the torpedoes being fired during the night?

Mr. Holmes: I quite agree with Professor Abel that it is quite right to have two strings to your bow; but do not rely upon the mechanical means. You can never have too many strings to your bow; but to let the defence rest entirely on mechanical appliances I consider is not desirable.

Major Malcolm, R.E.: I hope that Mr. Holmes won't think that I want to snatch an easy victory by talking about all these thousands of pounds that he began with; but I really was misled by this sentence, which states that the defence of the water approach to Richmond was intrusted to a single torpedo mine sunk in the channel way; and I thought that Mr. Holmes very probably was like a great many men. I did not know how deeply Mr. Holmes

had studied the question, but I know that there is a general idea that the thing will go off, and that a great many people will be killed, pretty much as in that picture.

Mr. Holmes: The defence of Richmond was intrusted to this single torpedo mine. That was the first mine laid in the river. The other torpedoes were laid closer up, and they did not come into action. The main defence was this large mine. There was no second mine put in the channel of the river to guard the approach.

Major Malcolm, R.E.: Then you see the defence was not intrusted to one mine. Then as to the few men that are to manage this. I do not know how you would get near to operate so many mines as you speak of, three hundred; you might, but how would they get on under the excitement of a fleet advancing in a good broad channel? Suppose they did not advance one by one, the men would have to do the best they could, as the ships would probably advance firing, for there would be a great deal of smoke. Then it would not be altogether easy for the observers to see and to agree upon a particular ship. That is a great matter. If a single ship comes there will be no doubt about it. It is very human to make mistakes, and I think the chances are strongly in favour of two observers when the fleet is pretty close to a mine fixing upon different ships. But leaving that alone you must give each man ten or twelve or possibly twenty mines; and to think that he is going to put his finger down upon thirty or fifty keys, at the critical moment is, I take upon myself to say, to expect more than a man can do. Therefore you must have many more observers. Then who is going to keep these 300 mines in order? I am perfectly aware that the great expense is the insulated cable, but it will get caught and damaged, and if there is nothing else there will be those fishermen going about, and who is to keep them in order? Therefore it would be absolutely necessary to have a large staff. Then in the paper Mr. Holmes says—(you will remember the passage)—that by means of torpedoes you are to defend the place beyond the range of cannon; and now when we come to handle the question a little bit we are obliged to withdraw our torpedoes under the protection of cannon. Well, that shows the advantages of this discussion on torpedo defence. *Then Captain* Maury, like everybody else,

wanted secrecy; and yet what would his electric light do when it was used at night but show the enemy where the torpedoes lay? Surely that would point out where they were like drawing a chalk line on the blackboard. You see there are such hundreds of difficulties about it.

Mr. HOLMES: Perhaps this diagram may not be properly understood. First of all, in the manipulation of a given number of torpedoes by this plan of Maury there is not any number of keys, there is only one key. This is a key that fires any number of mines, and it is moved according to the position or sweep of this telescope. One of these telescopes is placed in a station of observation, and when the torpedoes are first laid down, at the time that the torpedo is sunk, a mark is made on the degrees of the circle of the instrument. That would represent the position of one torpedo mine. A similar apparatus being placed in the other station, the observer there at the same instant of time also upon his apparatus makes a mark. Now it is quite obvious that, if a ship is crossing the line of that torpedo, the observer looking through his telescope will see the ship crossing the spider-line glass. Then he touches his handle, but, unless the ship is over the mine, the mine will not explode. Unless it is in line and the other observer depresses his handle at the same moment the circuit is not complete. The circuit being completed at B will not fire the mine unless the ship is actually over the torpedo and there is a concurrent depression of the handle at A completing the circuit. Therefore, in this system of defence no mine is exploded unless the ship is actually there. There is no running to a key in a hurry. If that trigger is on any of your points where you know that the torpedo is sunk, and you touch your key, the mine will explode if the ship is in position. If the ship is not in position the mine remains for the next time. That is what I mean by five or six men keeping a fleet at bay. It can only be by a system which is permanently carried out. You must have your observing stations and your mines accurately marked. Then you have your system complete. You cannot lay this down in one day. They must be properly planned.

The CHAIRMAN: If two or three ships advance at once, what is to prevent different ships from being chosen by the observers?

Mr. HOLMES: On these circuit-closers on land you would place

a certain number of mines. Every ship must go over each station before it comes into the next station, and it is in the power of every observer to close his circuit as it advances to the station. Of course if he misses any of the ships then they are in range of the next. As this diagram is merely an elementary diagram, I have simply put down two stations; but each of these stations may have three or four grouping machines of this nature, each one taking a separate series of defences. When one is passed then another comes into use. If one is missed then the observer comes to the next machines. You carry the defence forward as the vessel approaches, and as the ship comes to the next group of mines.

Major MALCOLM, R.E.: The diagram to which Mr. Holmes has been pointing is a picture of an instrument with which I am very well acquainted. If Mr. Holmes will do me the honour of coming down to Chatham I will draw him any number of diagrams of this kind, and show him how we use frictional, voltaic, dynamo-electric, magneto-electric, and other electrical modes of firing mines. But, that the meeting shall not go away with altogether a false impression, I wish to remark that there are only 360 degrees in ordinary circles; and that shows you at once that one of these instruments can only command a very few points, because, though your arrangement, your circuit-closer or trigger, or whatever you like to call it, takes up a certain space, you would be very apt to make little mistakes unless you have a great number of them; and that system there is one which, I am thankful to say, we have passed by and have improved upon. Then as to the elementary system which is there, there must be a battery in one station or another. Well, suppose a man puts his key down, and the other insulated cable is damaged, won't the circuit complete itself through the water? And if it does I do not know what will happen: at least, I *do* know what will happen. The mine will go off. You are entirely dependent upon the protection of your insulated cable.

Mr. HOLMES: This apparatus was elaborated by Maury himself. With reference to there being only 360 degrees in a circle, you can well use 90 of those degrees. You can take 20 or 30 groups of mines upon even one of these apparatus, because, when you are

observing a fleet, the fleet is advancing in one direction, and your glass is sweeping in one direction by a micrometer which makes your telescope keep pace with the steamer, the same as an astronomer keeps his telescope following a star. The observer has nothing to do but to keep his handle always at rest, and as the telescope sweeps over, and the ship is in line, if the trigger is depressed the mine would go off, provided that at the concurrent station there is also the same thing done. Therefore you can control 30 mines with each of these machines.

The CHAIRMAN: This has been a very interesting general discussion. It might have been even more interesting if we had more practical details to deal with, but to telegraphers a great many of the questions that have been discussed this evening are entirely novel and unknown. Telegraphy, however, has done one good thing for torpedo work. If I am rightly informed, Mr. Statham, in manufacturing gutta-percha wires in 1851 or 1852, and using sulphur with his gutta-percha, observed that the sulphur acted upon the copper, and formed a coating of sulphide of copper, and the passage of a very feeble current caused the iginition of this sulphide of copper and created fire. I think it was his discovery which first gave an impulse to the igniting of torpedo mines by electricity. Of course the platinum wire system was well known before that, but I think it was Mr. Statham who first called attention to this improved method of igniting with wire at great distances. I can only say that I have been very much pleased to hear that the Government are going to spend—or, perhaps, as a taxpayer I ought not to say that I am pleased that they are going to spend—£200,000.

Major MALCOLM, R.E.: They have spent it.

The CHAIRMAN: I very much doubt whether they have spent it; but I am glad to hear that they are alive on the subject, and that in case of our being attacked by any other nation on the earth we shall have such defences to take care of us. I have been amused at the manner in which the officials of the Government have been disposed to pitch into any outsiders who attempt to find fault with them. I can quite understand it. They are determined not to allow persons to sit on them. I can see that they are quite ready,

if attacked, to defend themselves, and I hope that if we are attacked they will be equally ready to defend us. I will now close this meeting by asking you to pass a very cordial vote of thanks to Mr. Holmes.

The following Candidates were balloted for and declared duly elected :—

As a MEMBER :—

Professor Noad, F.R.S. . .	St. George's Hospital.

As ASSOCIATES :—

Alfred Bennett . . .	91, Oxford Street.
John Gibson	Kirkwall, Orkney.
Henry Goodenough . . .	Great Western Railway, Paddington.
Robert Harrison . . .	The Grange, Ware.
Edward Warburton . . .	Hammersmith.

The Meeting then adjourned.

The Twenty-fourth Ordinary General Meeting was held on Wednesday, March 11th, 1874, Professor FOSTER, Vice-President, in the Chair.

The first Paper read was—

ON AN IMPROVED DOUBLE-CURRENT TELEGRAPH KEY.

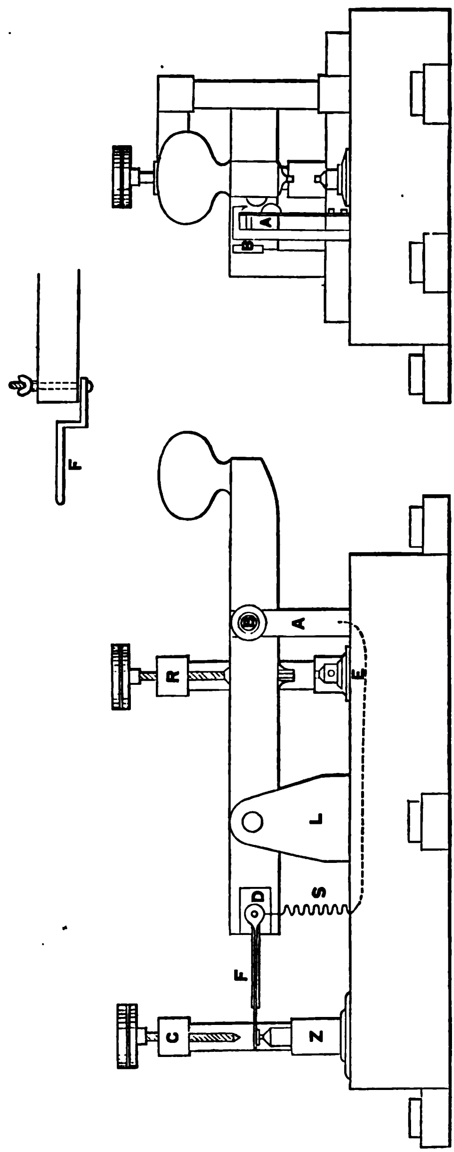
By J. J. FAHIE.

This instrument is intended to be used for signalling the Morse code through submarine cables, long landlines, or underground wires of moderate lengths, say 500 or 600 miles.

Its advantages over the double-current keys at present in use are—1st, a higher speed of signalling on long circuits—how high I have not yet determined—but certainly not less than 10 to 15 words per cent.; 2nd, the possibility of the receiving station stopping at pleasure the sending station during the transmission of a despatch; 3rd, its simplicity, and the ease with which it can be worked, as compared (for instance) with Siemens's submarine key; 4th, the no additional cost at which the great advantage of "stopping" is effected, in comparison with the present method of compassing the same end, viz., the automatic switch or zinc sender. This is a costly piece of apparatus, and easily gets out of adjustment, and thus causes some delay and confusion. But these are not its only drawbacks; being situated between the battery and the line, it not only adds to the electrical resistance of the latter, but, what is of more consequence, it reduces the speed of working.

My instrument, on the other hand, will not cost more than an ordinary double-current key, and will be as little liable to get out of order.

The accompanying diagrams exhibit a side and end view of the key. A is a spring contact, carrying near its free end a semicircular platinum piece of about a quarter inch diameter. It is in metallic connection with the tongue F, which is well insulated from the lever



J. J. Fahie's Double-Current Telegraph Key.

by the block of ebonite, or ivory, D. This tongue is secured by a small bolt which runs freely through the insulator, and is fastened at the other side by a nut E, curved steel washer. It plays between the contacts C and Z, and must remain firmly in the position in which it is placed. For this reason just sufficient pressure is put upon it by means of the nut and washer before mentioned, so that when it is touching the screw C it shall not fall of its own weight on to the contact Z. S is a strong spring for drawing up the lever to the position shown in plan I. It passes up through a hole in the ebonite; E is secured to the bolt. It is of course insulated from the lever. The other parts of the instrument need no description.

I will now explain its action. First suppose the key to be joined to a cable of (say) 200 miles. The play of the lever may in this case be reduced to half of that shown in plan. To effect this, the little screw B should be unscrewed until the spring is withdrawn to such a distance that the lever, on being depressed, shall only rub against a small arc of the contact piece A. The contact Z and the screw R are then adjusted so that the semicircular contact piece on side of lever is just clear above A when the lever is at rest. The lever is then depressed and the contact piece E adjusted until the lever contact is just below A. The screw C should always be as close to the tongue F as possible, and when the key is at rest the lever and R and the tongue F and Z should make good metallic contacts. The key is then "adjusted."

The copper pole of a battery of (say) 10 cells is connected to C, the zinc pole of another battery of equal strength to Z, the remaining poles being to earth. The line is connected to L, the earth to E, and the receiving instrument or relay to R.

Now to send a signal the lever is depressed; as soon as it moves downwards the connection between line and relay is broken, the tongue F immediately after leaves the zinc and goes on to the copper, and almost at the same moment the lever rubs over the spring contact A. While they are thus touching, a positive or copper current or wave flows out to line and closes the distant station's polarised relay. When the lever passes below A the battery is cut off, and when it touches E the line is put to earth.

On allowing the lever to rise after making the signal, the connection between line and earth is first broken, then the tongue leaves the copper and touches the zinc, and at the next instant A and the lever again rub together; while they are thus in contact a "zinc" wave flows out to line, and opens the distant relay. When the lever passes above A the battery connection is severed, and immediately after the line is put to the receiving instrument.

Positive and negative currents of equal strength and duration are thus sent into the line: the injurious effects of induction operate to a much less extent, and a greater speed is possible than when signals are made by currents of unequal strength and duration. (See 4th Edition of Culley's Handbook, paragraphs 468 and 469.) Again, by this arrangement the line is able to discharge itself after every positive and negative wave—a great advantage.

Should the distant station desire to "stop" the sender, the lever must be held for a second or two against the spring contact A, thus keeping a positive current on line. As soon as the sending station's lever returns to the contact R this positive current enters his relay and works his "Morse," whereupon the sender stops.

When working on longer lines of (say) 600 miles (and I do not think my key would work well through longer distances unless with more delicate relays than the old Red Sea pattern in use in the Persian Gulf) the contact spring A is allowed to fall forward to its full extent, so that the lever shall rub against a larger surface of the semicircular contact. For this the lever will require greater play, and the contacts R and E must be altered accordingly.

If at any time the receiving station complain of weak signals, it will be some help if the sending station throws off the earth wire from his key, and so compel the whole positive charge to go forward and act upon the distant station's relay; indeed, when working through *long or badly insulated* lines the earth-wire at E need not be used at all.

As a rule the battery power should be increased by about one-half—thus, if 10 cells be required to work with (say) Siemens' key, 15 should be employed with the new key.

My key may also be adjusted so as to send permanent positive currents followed by short negative currents. For this the earth-

wire should be removed and E elevated, so that the lever, when depressed, shall remain in connection with *a*; a permanent positive current thus passes out to line; while the lever is returning to its normal position after making each signal a negative current goes out. The strength or duration of this current can be altered within certain limits to suit the condition of the line by altering the play of the lever.

Worked in this manner the key will be found to give very satisfactory results on a long, well-insulated land-line.

Since August 1872 I have, on numerous occasions, tried this key on several of the sections of the Persian Gulf cables, and have always obtained very good results on lengths under 600 miles. With a more sensitive relay than the old and heavy ones available in this department, I am satisfied my key will work well with less play and through longer distances than at present.

The CHAIRMAN: This is a question of great practical importance, and the remarks of practical gentlemen present will no doubt give value to this communication.

Mr. C. W. SIEMENS: I did not understand the paper to state whether this key is intended to be worked in connection with the polarized relay.

The SECRETARY: Yes. The paper, I think, does state so.

Mr. C. W. SIEMENS: If so the key has the undoubted advantage of sending positive and negative currents of equal duration in order that the lines may neutralise each other, provided always that the motion downwards of the key is accomplished with the same velocity. If there should be any dragging in pushing the key down, or in allowing it to go up, then the time of the contact between the two pieces would be different on the downward journey from what it would be on the upward journey and the balance of current in the line would be vitiated. There are certain points in the construction which perhaps would present practical difficulties if the key left the hands of the inventor and passed into those of other operators. To begin with, the traverse

of the key is very considerable, and probably there might be an objection to working a key with that traverse; then the spring appears to require very neat adjustment. It has to exert an elastic pressure against the point C, on the point Z, and yet must be sufficiently free on the point D to turn after its own short stroke is accomplished. These are points of practical importance which probably may be modified, so that the objections I refer to may be overcome, but certainly the attempts here made to equalise the line currents appear to be meritorious. There is a good point in the relay. It will do away with the switch, holding the two contact pieces together, and thus interrupt the working on the other side. The saving of the switch must be regarded as a collateral advantage obtained by the key itself.

Mr. PHILLIPS: It seems to me that after sending the copper current into the line the key puts the line to earth; but that does not appear to occur after the zinc current is sent into the line. As far as I understand that seems to be a one-sided notion. The Siemens' key when brought back makes an earth contact, whatever current is sent.

Mr. LATIMER CLARK: I think this key does the same.

Mr. SIEMENS: We consider L equal to earth.

Mr. LATIMER CLARK: Can the Secretary tell us whether they are both alike in that respect?

The SECRETARY: It does not go to earth. The copper current discharges to line, and further depression of the key gives the zinc current, which goes into line and is partially neutralised. On the conclusion of the current the key goes back to its relay, its normal position. The paper was sent here from the Persian Gulf, and I have had no time to communicate with the author.

The following paper was then read—

Note on Mr. LATIMER CLARK'S METHOD OF MEASURING DIFFERENCES OF ELECTRIC POTENTIAL.

By Professor ADAMS.

In order to compare the electromotive forces of two batteries, M. Poggendorf joins them up in such a way that the current from the weaker battery may be balanced by an equal and opposite branch current from the stronger battery.

We may regard it as a method of determining in what length of line of a given simple circuit the fall of potential is equal to the electromotive force of the weaker battery.

Thus, if C be the electromotive force of the stronger battery, $R + \rho$ the total resistance of a simple circuit, and Q the strength of current, then by Ohm's Law $C = Q(R + \rho)$.

Now if the poles of another battery, whose electromotive force is E_1, be joined up as in Poggendorf's method to two points of the first circuit between which the fall of potential is equal to E_1, then no current will flow from this second battery, and the current in the simple circuit of the first battery will not be disturbed.

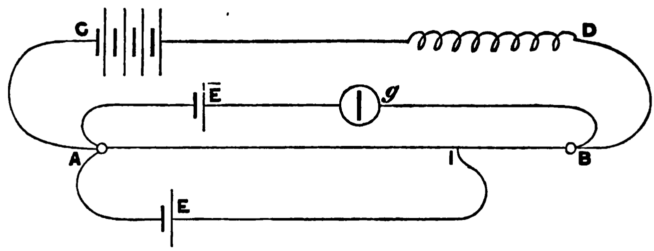
If we regard ρ as the resistance between these two points, then applying Kirchhoff's laws to the secondary circuit we get $E_1 = Q\rho$. Hence we obtain the result arrived at by Poggendorf, that

$$\frac{E_1}{C} = \frac{\rho}{R + \rho} \quad . \quad . \quad . \quad (a).$$

Mr. Latimer Clark employs this method, and greatly extends its use by determining in what length of line of the same simple circuit the fall of potential is equal to the electromotive force of a standard cell of known value. And the electromotive forces of the standard cell and another battery are proportional to the resistances between the points where their poles are joined up to the circuit of the first battery, so that no current may pass through them. The standard cell and the battery to be compared with it may be joined up at the same time to the principal circuit of the stronger battery, since their currents are completely balanced. In the same way it is clear that any number of separate batteries may be joined up to

the same principal circuit at the same time, and their electromotive forces compared directly with one another and with the standard.

Now let us consider how far a current in one of these secondary circuits will affect the other secondary circuits, which we may suppose to be already balanced.



Referring to the figure—

Let C A B D be the principal circuit of the principal battery C.

A E g B and A E_1 I be the secondary circuits, so arranged that each battery by itself would send a current in the same direction along the wire A B.

Let E be the electromotive force of the standard cell, and E_1 the electromotive force of the other secondary battery.

Assuming that currents flow in all the different branches—

Let Q be the strength of current in D C, R the resistance of A C D B, p_1 and p_2 the strengths of current in A I and I B respectively, q the strength of current in the standard cell, and q_1 the strength of current in the other cell E_1. Let ρ_1 and ρ_2 be the resistances of A I and I B respectively, γ and γ_1 the resistances of A E B and A E_1 I. Then applying Kirchhoff's laws to the primary and the several secondary circuits we get the following equations:—

$$C = QR + p_1 \rho_1 + p_2 \rho_2 \quad . \quad . \quad . \quad \text{(i.)}$$

$$E = q\gamma + p_1 \rho_1 + p_2 \rho_2 \, . \quad . \quad . \quad . \quad \text{(ii.)}$$

$$E_1 = q_1 \gamma_1 + p_1 \rho_1 \quad . \quad . \quad . \quad . \quad . \quad \text{(iii.)}$$

Also, since the algebraic sum of the strengths of current flowing to any point vanishes

$$Q - p_1 + q + q_1 = 0 \quad . \quad . \quad . \quad . \quad \text{(iv.)}$$

$$p_1 - p_2 - q_1 = 0 \, . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(v.)}$$

when the two secondary circuits are completely balanced, then equations (ii.) and (iii.) become

$$E = Q(\rho_1 + \rho_2), \text{ and } E_1 = Q\rho_1;$$

$$\text{therefore } \frac{E_1}{E} = \frac{\rho_1}{\rho_1 + \rho_2} \quad . \quad . \quad . \quad (\beta).$$

Now let us suppose that the standard cell E is balanced before E_1 is joined up to the primary circuit,

Then equations (i.) and (ii.) become

$$C = Q_1 R + p_1(\rho_1 + \rho_2) = Q_1(R + \rho_1 + \rho_2),$$
$$E = Q_1(\rho_1 + \rho_2) = p_1(\rho_1 + \rho_2),$$
$$\text{and } p_1 = p_2 = Q_1,$$

the relations being the same as when the circuit E_1 is completely balanced.

Now, under what conditions can E_1 be joined up so as not to send a current through the standard cell E?

These conditions may be found from the above five equations by making $q = o$.

If $q = o$, then from equations (ii.), (iv.), and (v.)

$$p_2 = p_1 - q_1 = Q,$$
$$\text{and } E = p_1\,\rho_1 + Q\,\rho_2;$$
$$\text{therefore } C = Q(R + \rho_2) + p_1\,\rho_1,$$
$$\text{and } C - E = Q\,R,$$

but C, E, and R remain the same;

therefore $Q = Q_1$, the same as before;

hence from (iv.) and (v.), $p_1 = p_2 = Q_1$ and $q_1 = o$;

so that when the standard cell is balanced and an additional secondary circuit is joined up at A and at any point I, so as to produce a current through E_1, then a current will also be produced through the constant cell.

Thus it appears, as might have been expected, that a current through any secondary circuit will produce a current through the constant cell. Hence to compare the electromotive forces of any number of separate secondary circuits it is only necessary to have one galvanometer, which should be placed in the circuit of the standard cell.

The direct relation between the currents produced at the same time in two secondary circuits by altering the point of junction I

of one of them may be derived from the above five equations, remembering that R, γ and γ_1, and also $\rho_1 + \rho_2$, remain constant. Let $\rho_1 + \rho_2 = k$.

From (i.) and (ii.) we get $C - E = Q\,R - q\,\gamma \ldots\ldots (\gamma)$

From (i.) and (iii.) $\quad C - E_1 = Q\,R - q_1\gamma_1 + p_2\rho_2$

Also from (iii.) and (v.) $E_1 = q_1\gamma_1 + q_1\,\rho_1 + p_2\,\rho_1$

Also from (ii.) and (iii.) $E - E_1 = q\,\gamma - q_1\gamma_1 + p_2\,\rho_2$.

Eliminating p_2 from these two last equations we get

$$E\,\rho_1 - E_1\,(\rho_1 + \rho_2) = q\,\gamma\,\rho_1 - q_1\,\rho_1\,\rho_2 - q_1\,\gamma_1\,(\rho_1 + \rho_2)$$

$$\text{or } (E - q\,\gamma)\,\rho_1 - q_1\,\rho_1{}^2 + q_1\,\rho_1\,k = (E_1 - q_1\,\gamma_1)\,k \ldots . (\delta)$$

This equation (δ) connects q, q_1, and ρ_1.

Another equation connecting the same quantities may be obtained from equations (iii.), (iv.), and (γ).

Thus $E_1 = Q\,\rho_1 + q\,\rho_1 + q_1\,(\gamma_1 + \rho_1)$,

And $E_1\,R - (C - E)\,\rho_1 = q\,\rho_1\,(R + \gamma) + q_1\,R\,(\gamma_1 + \rho_1)$

$$\text{or } (C - E)\,\rho_1 + q\,\gamma\,\rho_1 + (q + q_1)\,R\,\rho_1 = (E_1 - q_1\gamma_1)\,R \,.\,. (\epsilon).$$

The two equations (δ) and (ϵ) give the strengths of currents in the two secondary circuits when the point I is shifted along the line A B in terms of the resistance of A I.

If, then, we suppose the two secondary circuits to be balanced, these equations give the strengths of the two currents due to a change of position of the point of contact I. And the equation (γ) gives the consequent change of current through the primary battery. In this case we have the additional relation

$$\frac{E}{C} = \frac{k}{R + k},$$

$$\text{or } \frac{E}{k} = \frac{C}{R + k} = \frac{C - E}{R}.$$

Hence, dividing equation (δ) by k and (ϵ) by R and subtracting, we get

$$\rho_1\left\{q\gamma\left(\frac{1}{k} + \frac{1}{R}\right) + q + q_1\frac{\rho_1}{k}\right\} = 0,$$

$$\text{or } \frac{q}{q_1} = \frac{\rho^1}{k\left\{1 + \gamma\left(\frac{1}{k} + \frac{1}{R}\right)\right\}} \cdot \cdot \cdot \cdot \cdot \cdot \quad (\xi).$$

The important practical result which we arrive at by con-

sidering these equations is this, that in order to compare the electromotive force of any battery with the standard cell it is only necessary to employ one galvanometer, and that, if this galvanometer be joined up with the standard cell, the electromotive force of one or of any number of separate batteries may be found by shifting the point of contact I of each one of them until no current is produced through the galvanometer when the contact at I is made or broken.

Supposing that the resistance of the wire A I in this arrangement is an unknown resistance, the resistance I B being known, then, by attaching the poles of a quadrant electrometer at A and I instead of the battery E_1, we may determine the resistance of the wire A I by means of the difference of potential between the two points, as shown by the electrometer; the joining of the electrometer poles at A and B showing the deflection corresponding to the standard cell.

Mr. Latimer Clark: I must say I think Professor Adams has made a very useful and practical simplification of the old method of measuring potentials, and one which I think will be more and more employed the more it becomes known. It is a most convenient method, and ought to be more generally used than it has hitherto been. I very much like the suggestion which Professor Adams has made, that we could measure the resistance by reversing the operation and using the electrometer as well as being able to measure the potential. I hope Professor Adams will give us some better idea as to how far practically we can depend upon the use of one galvanometer only; that is to say, whether we can measure potentials a hundredth, two hundredth, or five hundredth of the whole, with an ordinary practical galvanometer. No doubt his calculations will enable him to give us some idea as to within what percentage we may rely upon the measurement of potential of the smaller battery. Of course it was impossible for me, in conducting experiments, not to see the relation of movement between the two galvanometers. Without having the ability to calculate the

relation, that being a very complex matter, I judge merely from my own ideas, that it would not be sensitive enough, but the idea occurred to me that it might have been so used, as I find it is constantly necessary to watch the two galvanometers and to keep them adjusted. I may also mention that I have used the instrument for obtaining an exact and known degree of potential in small condensers. It is a very convenient means of giving an exact unit measure in them. I have found it possible to divide the potential of a single cell of any ordinary battery, such as a Daniell's battery, into a million equal parts. That is done by using a Thomson's galvanometer. I will not say I can always rely upon such measurements, but I can certainly indicate a difference of a millionth part. Measurements of a thousandth part can be read and determined with very great accuracy; but if Professor Adams will tell us whether we can come within one-tenth per cent. or one per cent. of the real measure it will be a matter of practical utility.

Professor ADAMS: My formula is not sufficiently reduced to numbers to enable me to give at once the relation between the currents flowing in the two branch circuits for any particular case. I have the relation contained in the equation (ξ), which shows that the sensibility of the method depends on the relative values of the resistances of A I and A B, and also on the resistance of the standard cell. A practical way of determining the question would be to make use of two galvanometers and note accurately the deflections of both at the same instant. I should rather try the experimental method for determining the sensibility and see whether it agreed with the mathematical result, noting the deflections produced in the two galvanometers; by shifting the point of connection through a given resistance the result would be at once obtained. Experimentally it would be a short process.

Mr. LATIMER CLARK: My impression is from what I remember that I was able to measure accurately to about the thousandth part of a Daniell's cell with a resistance of 40 ohms in the wire. I have applied the instrument first in a small portable form—a single bar 6 feet long—and with that I found no difficulty in dividing a Daniell's cell into a thousandth part. We could then ascertain the distinct changes in the galvanometer.

The CHAIRMAN: There is one point mentioned by Professor Adams which struck me as of special importance—that is, that we shall be able to compare not only the two, but any number of batteries at the same time, with the use of a single galvanometer. It must be inconvenient in most laboratories to employ many galvanometers at once for such a purpose, and I can well imagine cases where it might be desirable to compare simultaneously several electromotive forces. It therefore strikes me, on a general view of the subject, that the comparison will be very accurate when the electromotive forces to be compared are nearly equal, but that the sensitiveness of the arrangement will be considerably diminished if they differ very much. If the two points of connection, B and I, are near together, the variation of one should affect the other equally. If far apart it would not do so to the same extent.

Mr. LATIMER CLARK: Undoubtedly.

Professor ADAMS: There is no doubt the arrangement is most sensitive when the resistances are nearly equal, but unless the difference is very great the method will give a very close approximation to the result which would be obtained by inserting a second galvanometer.

Take a particular case to test the sensibility of the method.

If in the principal battery C there are 10 cells, each of ·5 ohms resistance, the resistance of the rheostat D being 40 ohms, the resistance of the galvanometer and shunt 1 ohm, and if one similar cell is used as the standard,

$$\text{then } R = 45 \text{ ohms}, \gamma = 1{\cdot}5, \text{and } \frac{R + k}{k} = 10.$$

$$\text{Then } \frac{q}{q_1} = \frac{\rho}{k\,(1 + \frac{1}{3})},$$

making the sensibility about 75 per cent. of what it would be with two galvanometers when the secondary batteries are nearly equal. By increasing the resistance of the rheostat D, we may make the method far more sensitive.

$$\text{If } D = 400 \text{ ohms}, \gamma\left(\frac{1}{k} + \frac{1}{R}\right) = \frac{1}{27},$$

making the sensibility about 96 per cent. of what it would be with two galvanometers.

When the electromotive forces of the secondary batteries differ very widely from one another, the sensibility of the method will be expressed by the ratio of the electro-motive forces. In such cases, if only one galvanometer be used, it should be placed in the same branch circuit as the battery which has the least electromotive force.

The following paper was then read:—

CONDENSERS IN CONNECTION WITH DUPLEX TELEGRAPHY.

By Mr. R. S. Culley, Vice-President.

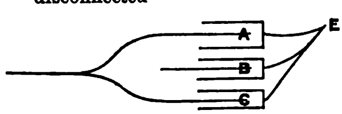
It has been found that in using condensers with duplex telegraphy the alteration of the capacity of the condenser sometimes made little or no difference in the working of the circuit. This was pointed out to me by Mr. Stearns with his own conjectures as to the cause; he remarked, "Mr. Marson told me that it seemed to matter very little whether two or five microfarads were used on some circuits. This sorely puzzled me. Mr. Carson at the Anglo-American made the same report. I tried to test the question at Brighton when using one of your condensers, and again when trying to work Hughes's duplex between the General Post Office and Southampton some days ago. My own observations confirm Mr. Marson's."

Mr. Stearns attributed the cause to an inductive action between plate and plate in the condenser, making the aggregate action complicated and unsatisfactory.

In order to test this view of the question a numerous and careful series of experiments were made for me by Mr. Andrew Bell at the Gloucester Road Stores, and, as they are of an interesting character and clear up this point, I have considered them as being not unacceptable to the Society.

Before, however, describing these experiments it would be useful to point out the kind of condenser referred to, and its mode of manufacture. The introduction of duplex telegraphy has necessi-

Measurement of Condenser-plates Detached and Superposed—Capacity of the Three Plates used about 1 Micro-farad each.

Plates or Combination and Position of Line and Earth Connections.	Charge.	Immediate discharge on a short Contact.	Immediate discharge on Contact, giving full saturation.	Time.	
	Mean.	Mean.	Mean.	Time.	No.
	degrees	degrees	degrees		
Plate A detached	100·5	96·5	100	3″	1
„ B „	97	93·5	97	„	
„ C „	100·5	95·5	100	„	
Sum	298	285·5	297		
Plates A, B, and C, superposed as in box	291·4	282·4	293·7	5″	2
„ A and C, Line connection of B disconnected	197	191	197·6	4″	3
					
„ A and C, Line and Earth of B disconnected	197	191·6	197·6	„	4
„ B, Lines A and C disconnected	93	93·3	96	3″	5
„ B, Lines and Earth A and C disconnected	94	93·3	96	„	6

The inductive dimensions of foil are = 39·5 sq. inches—18 times in inside foil=4·93 sq. ft.=9·86 sq. feet=1 microfarad capacity. Some plates require as much as ¼ additional surface to give one microfarad capacity; the difference being due to the varying thickness of paper, and the varying distance between the foils due to alteration of the heat of the surface-plates between which the plate is pressed.

From the foregoing figures it will be seen that the total reading from the three plates differed but little from the sum of the

reading of the three plates taken separately. There appeared to be no difference when the line only of one plate (the middle one) and when the line and earth were disconnected. The sum of the two plates gave 197·5°, the two plates taken together giving 197°, and 197° when the plate B was disconnected, showing that the disconnection of B had no effect upon the discharges obtained from the plates on each side of it. Taking also the discharges obtained from B alone with plate A on one side of it and plate C on the other, there is no difference to warrant a supposition that there was an inductive influence at work tending to alter the capacity of the condenser.

In order to assimilate these experiments as much as possible to the ordinary duplex working, some were taken with split strong currents, to obtain as near as possible the discharges resulting from duplex working. They were as follows :—

MEASUREMENT OF CONDENSER.

Discharge measured.—Charge split through 10,000 ohms.

Plates or Combinations, and Position of Line and Earth Connections.	No. of Charging Cells.	Immediate Discharge on a short contact.	
		Separate Measurements.	Mean.
Plate A detached	50	96, 97, 97, 97	96·7
„ B „	„	94, 94, 93, 93	93·5
„ C „	„	94, 95, 95, 94	94·5
		Sum	284·7
Plates A B and C superposed .	„	285, 282, 283, 284	283·5
„ A and C, Line of B disconnected	„	192, 191, 191, 192	191·5
„ B, Lines of A and C disconnected	„	94, 93, 94, 94	93·7

Plates or Combinations, and Position of Line and Earth Connections.	No. of Charging Cells.	Immediate Discharge on a short contact.	
		Separate Measurements.	Mean.
Plate A detached	100	96, 97, 96, 97	96·5
„ B „	„	93, 94, 93, 93	93·7
„ C „	„	93, 94, 94, 94	93·7
		Sum	283·9
Plates A B and C superposed .	„	281, 282, 281, 282	281·5
„ A and C, Line of B disconnected	„	190, 190, 189, 190	189·7
„ B, Lines of A and C disconnected . . .	„	93, 93, 94, 94	93·5

From the above it will be seen how closely the various results agree. The very slight difference due in one or two cases must be attributable to a little longer or shorter duration of the momentary contacts as used in signalling.

A further experiment was tried which was somewhat conclusive. The three plates, A, B, and C, were superposed, B being as in each case in the middle; A and C were charged in the usual manner, but there were no signs of any induced charge in B.

The second series of experiments were taken with a finished condenser (the first series being with loose plates). To enable the charging current to be properly split, the connections of a double current key were altered to suit the purpose, so that short contacts made with it should resemble actual duplex working as near as possible.

When measuring this condenser there was fortunately little or no disturbance from engines in the Camden Yard: repeated readings scarcely varied a single division on the scale. The plates are placed in the case in the same order as that of the measurements given below, and the measurement of the plates before they were placed in the case is also shown. The plates sometimes lose a little capacity, owing to the filling up paraffin loosening the tinfoil at the edges, and sometimes they gain from the smoothing down of cut foils.

Condenser No. 52—1 microfarad = 300 divisions.

			In case.	Before placed in case.
Plate A = 2	m.f.	=	616°	608°
„ B = 1	„	=	308°	300°
„ C = 0·5	„	=	160°	155°
„ D = 0·25	„	=	79°	78°
„ E = 0·125	„	=	38°	37°

With shunt altered so that B plate = 100 divisions, the measurements separately were as follows:

Plate A = 200					
„ B = 100	- 300				
„ C = 52	-	- 352			
„ D = 26	-	-	- 378		
„ E = 13	-	-	-	- 391	

Then, arranged in groups, gave following:

Plates A + B	= 300				
„ A, B, and C	=	- 353			
„ A, B, C, and D	=	-	- 379		
„ A, B, C, D, and E	=	-	-	- 392	
„ A, C, and E	= 256 (sum 265)				
„ B and D	= 122 („ 126)				

The above measurements were made with only a potential of 4 cells, and the plates allowed to charge fully.

The following tests were taken with 50 cells split through a large resistance (10,000 ohms), shunt being altered to give same reading for the plates. The plates of course take full charge much quicker from the higher potential, yet a short signalling contact does not fully charge, and, as might be expected, the decrement is proportional to the capacity. In the ·125 microfarad plate (E) there was no apparent decrement because of the low constant, but in the microfarad plate the decrement was about 4°/₀.

Plate A = 192					
„ B = 97	-	- 289			
„ C = 50	-	-	- 339		
„ D = 25	-	-	-	- 364	
„ E = 13	-	-	-	-	- 377

Plates A and B	=	289			
„ A B and C	=	-	339		
„ A B C and D	=	-	-	364	
„ A B C D and E	=	-	-	-	376
„ A C and E	=	256			
„ B and D	=	122			

The following statement gives some particulars of the duplex circuit on which condensers are used. It will be seen that the lengths of these circuits vary from 51 miles to 400, and the capacity of the condensers in actual use from 0·5 microfarad to 10·75, which last is at Dublin at the end almost of the Holyhead cable.

CAPACITY OF CONDENSERS ON DUPLEX CIRCUITS.

From	To	Length.		Capacity of Condensers in inches.	
		Miles.	Gauge.	From	To
				Microfarads.	
Telegraph Street .	Birmingham	130	10	2·75	
Ditto .	Ditto .	130	10	2·40	
Ditto .	Manchester	192	8	3·00	3·0
Ditto .	Ditto .	208	8	2·75	3·0
Ditto .	Edinburgh .	400	8	3·00	
Ditto .	Ditto .	400	8	3·48	
Ditto .	Liverpool .	205	8	2·75	2·0
Ditto .	Ditto .	205	8	3·24	3·0
Ditto .	Dublin .	370	4	2·5 3·0	10·75
Ditto .	Bristol .	120	8	1·0	
Edinburgh . .	Liverpool .	227	8	1·0	
Ditto .	Dundee .	51	8	0·5	0·5
Glasgow . .	Inverness .	210	8	2·75	4·0
		2,848		34·12	

Mr. Latimer Clark: The paper only confirms what might have been very well expected from theory. It would have been most surprising if there had been any induction taking place between two plates of tin foil which are separated by a third plate which is in connection with earth. At the same time it is satisfactory to find this view confirmed by a series of carefully conducted experiments, and to know that the condensers may be relied on as true and good measures of electrical capacity. There however seems to be some difficulty in getting a permanent condenser, and so far as I am aware it has not yet been accomplished. I have tried mica instead of paper, and shellac, paraffin, and various compounds of gutta-percha, but nothing has been quite permanent. In 1862 I made three condensers very carefully of shellac and mica, and they were adjusted very accurately together. They were used in India and various parts of the globe, and they remained quite coincident until about three years ago, when they began to change in their relative capacities, and at the present time one is 6 or 7 per cent. below the others, and the one of the largest capacity is the most accurate. With gutta-percha and paraffin the changes were much greater. A permanent condenser which would retain a fixed measure of electricity is a great desideratum.

Mr. Phillips: May I ask whether the paper Mr. Culley uses is rag paper? In some experiments which I tried some years ago I found that a very common kind of straw paper gave greatly the best results. I judge from the appearance of this paper it is rag paper.

Mr. Latimer Clark, after an examination, pronounced it to be rag paper.

The Chairman: I have to ask you now to express your thanks to the authors of the papers which have been read to-night, which have been of great value. It is right that we should remember that Mr. Culley, in communicating his paper, has set an example worthy of imitation by others occupying a similar position, in having made public for our benefit the results which he has obtained in his official investigations, which might otherwise have been buried, without any one having any right to complain, in the records of the Post Office.

The following Candidates were balloted for and declared duly elected:—

As MEMBER:—

R. Vicars Boyle, C.S.I. . .	Yokohama, Japan.

As ASSOCIATES:—

John Donald	Postal Telegraphs, Stranraer.
Edwin Laurie . . .	Postal Telegraphs, G.P.O.
Alex. McKinney . . .	Postal Telegraphs, Manchester.
Edwin Wilde . . .	Postal Telegraphs, Leeds.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

ON THE USE OF ELECTRO-MAGNETIC INDUCTION IN CABLE SIGNALLING.

By G. K. WINTER, F.R.A.S.,
Telegraph Engineer, Madras Railway.

It will doubtless be remembered by most Telegraph Engineers that at the Brighton meeting of the British Association I brought forward a subject connected with signalling on submarine cables that I thought had not received the attention it deserved. This subject was the use of an induction coil at the receiving end of a cable, the current from the cable being made to pass through the primary wire, and the currents generated in the secondary wire being used for working the receiving instrument.

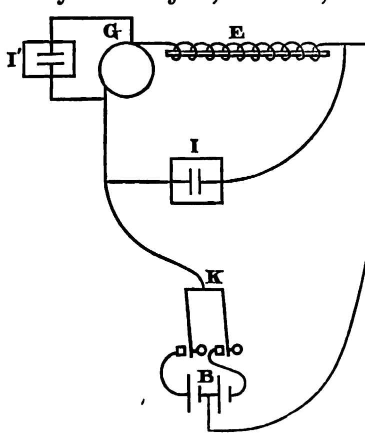
There was one point connected with my experiments, and passed over as a matter of course in my paper, which seems, nevertheless, to have been misunderstood—I refer to the fact that, having a primary wire adapted as to its length and resistance for making the most of the received current, you may fill up the space to be occupied by the secondary wire with wire of any diameter, so long as you use a galvanometer or other receiving instrument of an appropriate resistance; in other words, the secondary coil must be treated as a distinct rheomotor, and, therefore, according to a well-known law, the resistance of the galvanometer should be equal to that of the secondary coil.

The resistance of my primary wire was about 2,500 units, and, as explained in my paper, the resistance of the secondary coil and the galvanometer were about 2·5 units each. It is manifest that signals even from a strong battery, if passed through, say, 20,000 units, would scarcely affect such a galvanometer; whereas, if the same signals were passed through the primary wire of the above-mentioned induction coil, this same galvanometer would be specially

adapted to show the currents generated in the thick secondary wire. A galvanometer, however, of the same dimensions, wound with 2,500 units of fine wire, would be far more strongly affected by the direct signals than the thick wire galvanometer would be, even when joined to the secondary wire of the induction coil.

The induction coil is not, therefore, able to increase the obtainable strength of signals, but, if wound as mine was, it is able to convert signals that require a high resistance instrument to show them into others that are best shown by one with low resistance. There are some instruments that are essentially low resistance instruments, and which cannot be modified in this respect as galvanometers can. Such instruments would, of course, be useless as direct receiving instruments from long circuits, but would be rendered available for this purpose by means of the induction coil, just as my galvanometer of 2½ units resistance was in my experiments in April 1872.

My chief object, however, in writing the present paper, is to make known an important improvement I have made in the use of the induction coil in cable signalling. The great disadvantage in the use of the induction coil is the so-called *magnetic retardation* experienced by the cable current in passing through the primary wire. This magnetic retardation is caused by self-induction in the primary wire; any change in the current passing through the wire tends to produce a current in the opposite direction to such a change, and in this way rapid changes are, as it were, clogged, the effect being very similar to an increase in the length of the cable; and in this way magnetic retardation seems a very appropriate name for expressing the effect.



A pretty experiment, showing the effect of this retardation, may be made in the following way:—

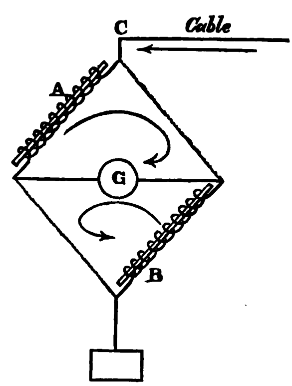
Let G, fig. 1, be an ordinary mirror galvanometer, with a pretty

thick fibre; E an electro-magnet, consisting of a large bar of iron, wound with, say, 2,500 units of No. 30 silk-covered copper wire; I and I′ are induction batteries, consisting of lead plates in diluted sulphuric acid; K a key with two contact makers; and B a battery. If, now, the key is worked, signals will be shown on the galvanometer closely resembling those received from a cable. This method of making a cheap artificial cable has been very useful to me in my experiments, and most probably may be useful to others. I may add, that a simple reversal of these arrangements, placing the induction batteries in the main circuit and the electro-magnet as the shunt, produces an effect analogous to what takes place at the sending end of a cable, and this constituted one of my first methods of forming a compensating circuit for applying duplex telegraphy to cables.

Now replace the electro-magnet by resistance of the same amount as the wire of the electro-magnet, and the character of the signals is immediately changed. The retardation is considerably lessened, and the effect is similar to using a much shorter cable.

This magnetic retardation is the chief drawback to the use of the induction coil. I have, however, been able not only to eliminate it but to cause the self-induced currents which are its cause to aid in the formation of signals.

The method is briefly as follows:—The primary and secondary wires of an induction coil form two alternate branches of a Wheatstone's Bridge, say, A and B, fig. 2, are these branches. The other branches are simple resistance, which may be made to produce balance when a constant current is flowing. G is the galvanometer or other receiving instrument. The current entering at C divides between the resistance and the primary wire. The increase of the current through the primary wire A not only induces a current in B, the secondary wire, in the direction shown by the lower arrow, but also causes a self-induced current to flow in the direction of the upper arrow; again, the



increase of the current through B not only causes an induced current in A in the direction of the upper arrow, but also causes a self-induced current to flow in the direction of the lower arrow. During the decrease of the cable current the direction of the induced currents is reversed. It is this reversal of the induced currents during the decrease of the cable current which gives value to the induction coil in cable signalling. Now, the self-induced currents, which in my plan aid the formation of signals, are the very cause of magnetic retardation in the ordinary way of using the induction coil.

One great advantage in the use of the induction coil over the condenser plan is the much greater safety the cable is placed in during the prevalence of those intense earth currents which accompany magnetic storms. In my paper on earth currents I showed that the difference of potential between the conductor and the earth was the greatest possible when only one end of the cable was insulated, and least, or rather nil, when both ends were connected to earth. Now, in the way in which cables are usually worked at present, the sending end of the cable is either connected direct to earth or through a small battery to earth, while the other end is insulated by the condenser. In this way the cable is, at the receiving end, submitted to the greatest strain possible. On some lines, however, the cable is kept completely insulated between two condensers, one at each end. It is not difficult to show that at each end of an insulated conductor the electrical strain (if we may so call it) between the conductor and the earth, produced by earth currents, would be just half that which would be produced at an insulated end when the other end is joined to earth. When using the induction coil, or my modification of it, as a receiving apparatus instead of the condenser, the cable is joined to earth at each end through a moderate resistance, and is therefore nearly in its safest possible state.

INDIAN TELEGRAPH IRON WIRE GAUGE.

The following particulars of the Iron Wire Gauge have lately been issued by the Government, and are now printed for the information of members of the Society:—

1. The Indian Telegraph Iron Wire Gauge depends on the weight per mile of the wire, from which the resistance, breaking strain, and comparative strain upon the insulators or posts can be all readily calculated in terms of a unit size.

2. Its unit* is a wire weighing 25 lbs. per statute mile.

3. All other sizes are known by their multiple of the unit.

Thus any gauge n weighs $n \times 25$ lbs. per mile.

4. The diameters may be calculated on the supposition that a rod 1″ in diameter and 1 mile long weighs 13,833·6 lbs.

Hence the diameter of any gauge n of wire is

$$\sqrt{\frac{\text{weight per mile}}{13833{\cdot}6}}$$

$$\text{or} \quad \sqrt{\frac{n \times 25}{13833{\cdot}6}}$$

$$\text{or} \quad \frac{\sqrt{n}}{23{\cdot}5} \text{ nearly.}$$

5. The sectional area of any gauge n

$$= d^2 \times \frac{\pi}{4}$$

$$= \frac{n \times 25}{13833{\cdot}6} \times \frac{\pi}{4}$$

$$= n \times \frac{25 \times 3{\cdot}1416}{13833{\cdot}6 \times 4}$$

$$= \frac{n}{704{\cdot}54} \text{ sq. inches nearly.}$$

6. A long series of tests has shown that the average resistance

* It has been proposed that the unit should be a wire of 100 lbs. per mile, and the sizes known by the multiples of hundreds and fractions. Thus wire weighing 750 lbs. per mile would be $7\frac{1}{2}$. This plan, however, although possessing the advantage of conveying a definite idea of size, was found to neutralize the simplicity of all the connected equations.

of wire weighing 600 lbs. per mile, or No. 24, is about 10·5 ohms per mile at 80° F.

Hence, as the resistance of wire varies inversely as its weight, the resistance per mile of any gauge n of wire at 80° F. will be

$$\frac{24}{n} \times 10{\cdot}5$$

$$= \frac{252}{n} \text{ ohms.*}$$

7. For wire of a quality equal to that of the Indian Standard Specification, the breaking strain (which varies as the sectional area and therefore as the weight) is equal to the weight of $3\frac{1}{3}$ miles of itself,

$$\text{or} \quad = \text{lbs. } \frac{1000}{12} n.$$

8. As wire is generally erected at $\frac{1}{4}$ breaking strain, the working strain on the wire, and the strain on a terminal insulator, will be

$$= \text{lbs. } \tfrac{1}{4}\left(\frac{1000}{12}\right) n$$

$$= \text{lbs. } 21 n \text{ nearly.}$$

9. At an angle post, where ϕ is the supplement of the angle contained by the wire, the resultant strain on the insulator is

$$2 \times \text{working strain of wire} \times \cos \frac{180° - \phi}{2}$$

$$= 2 \times 21 n \times \cos\left(90° - \frac{\phi}{2}\right)$$

$$= 42 n \sin \frac{\phi}{2} \text{ lbs.}$$

This form is the most convenient if a table of sines be at hand. If, however, such a table be not available, then the following expression for the resultant strain can be employed, viz.—

$$R = 21 n \sqrt{2(1 - \cos\phi)} \text{ lbs.}$$

and the cosine can be found by direct measurement on the ground, as follows:—

Measure 1000 inches = 83′ 4″ with a tape from the angle post

* It is to be remembered that, according to Mathiessen, the resistance will vary ·39 per cent. per degree Fahrenheit.

in the direction of the old alignment, and from the point thus found lay off a perpendicular intercepting the new alignment. Let x be the number of inches in the perpendicular intercepted between the two alignments. Then, as $\tan = \frac{x}{1000}$, the direct measurement of x in inches will give the tangent correct to 3 places of decimals.

But
$$\cos = \frac{1}{\sqrt{1 + \tan^2}}$$
$$\therefore \quad \cos = \frac{1}{\sqrt{1 + \frac{x^2}{10^6}}}.$$

10. It will be found by calculation that 1 ton of wire weighing 25 lbs. per mile is 157,696 yards or 89·6 miles long.

Hence

1 ton of any gauge n of wire measures $\frac{89{\cdot}6}{n}$ miles,

1 cwt. " " " $\frac{4{\cdot}48}{n}$ miles;

or, allowing a working margin for waste, &c.,

1 ton will pay out $\frac{85}{n}$ miles,

1 cwt. " " $\frac{4{\cdot}25}{n}$ "

11. The accompanying table gives every possible detail that can be required for every size from No. 1 to No. 50, but it must be merely considered as a list of calculations that have been made, and not as the table of the gauge. It need never be referred to if the above simple formulæ be remembered. These are now repeated for ready reference.

I. Weight per mile $= 25\,n$ lbs.

II. Diameter $= \frac{\sqrt{n}}{33{\cdot}5}$ inches nearly.

III. Sectional area $= \frac{n}{704{\cdot}5}$ square inches nearly.

IV. Resistance at 80° F. $= \dfrac{252}{n}$ ohms. per mile.

V. Breaking strain $= \dfrac{1000}{12} n$ lbs. or $3\frac{1}{3}$ its weight per mile.

VI. $\left.\begin{array}{c}\text{Strain on a Terminal}\\ \text{Insulator}\end{array}\right\} = 21n$ lbs. nearly.

VII. $\left.\begin{array}{c}\text{Strain on an Angle}\\ \text{Insulator}\end{array}\right\} \begin{array}{l} = 42n \sin \dfrac{\phi}{2} \text{ lbs., or} \\ = 21n \sqrt{2(1 - \cos \phi)}. \end{array}$

VIII. 1 ton pays out $\dfrac{85}{n}$ miles.

1 cwt. „ $\dfrac{4{\cdot}25}{n}$ miles.

H. MALLOCK,
Director of Construction.

Approved,
D. G. ROBINSON, Col. R.E.,
Director-General of Telegraphs in India.

Indian Telegraph Iron Wire Gauge.

Gauge. No.	Weight in lbs. per Mile.	Weight in lbs. per Yard.	Weight in lbs. per Foot.	Diameter in $\frac{1}{1000}$ inches.	Area of section in $\frac{1}{10000}$ sq.-inches.	Breaking strain in lbs.	Resistance per mile in ohms at 80° F.	Ratio of the resistance in terms of No. 50 wire.
1	25	·0142	·0047	42	14	83	252·00	50·000
2	50	·0284	·0095	60	28	167	126·00	25·000
3	75	·0426	·0142	75	43	250	84·00	16·667
4	100	·0568	·0149	85	57	333	63·00	12·500
5	125	·0710	·0237	95	71	417	50·40	10·000
6	150	·0852	·0284	104	85	500	42·00	8·333
7	175	·0994	·0331	112	99	583	36·00	7·143
8	200	·1136	·0379	120	114	697	31·50	6·250
9	225	·1278	·0426	127	128	750	28·00	5·556
10	250	·1420	·0473	134	142	833	25·20	5·000
11	275	·1562	·0521	140	156	917	22·91	4·545
12	300	·1705	·0568	147	170	1000	21·00	4·167
13	325	·1847	·0616	153	184	1083	19·56	3·846
14	350	·1989	·0663	159	198	1167	18·00	3·572
15	375	·2131	·0710	166	213	1250	16·80	3·334
16	400	·2273	·0758	170	227	1333	15·75	3·125
17	425	·2415	·0805	175	241	1417	14·82	2·941
18	450	·2557	·0852	180	255	1500	14·00	2·778
19	475	·2699	·0900	185	270	1583	13·26	2·632
20	500	·2841	·0947	190	284	1667	12·60	2·500
21	525	·2983	·0994	195	298	1750	12·00	2·381
22	550	·3125	·1042	200	312	1833	11·45	2·273
23	575	·3267	·1089	204	326	1917	10·96	2·174
24	600	·3409	·1136	208	341	2000	10·50	2·084
25	625	·3551	·1184	212	355	2083	10·08	2·000
26	650	·3693	·1231	217	369	2167	9·78	1·923
27	675	·3835	·1278	221	383	2250	9·33	1·852
28	700	·3977	·1326	225	397	2333	9·00	1·786
29	725	·4119	·1373	229	412	2417	8·67	1·724
30	750	·4261	·1420	233	426	2500	8·40	1·667
31	775	·4403	·1468	237	440	2583	8·13	1·613
32	800	·4545	·1515	240	454	2667	7·87	1·563
33	825	·4687	·1562	244	468	2750	7·64	1·515
34	850	·4830	·1610	248	483	2833	7·41	1·471
35	875	·4972	·1657	251	497	2917	7·20	1·429
36	900	·5114	·1704	255	511	3000	7·00	1·389
37	925	·5256	·1752	258	525	3083	6·81	1·352
38	950	·5398	·1799	262	539	3167	6·63	1·316
39	975	·5540	·1846	265	554	3256	6·46	1·282
40	1000	·5682	·1894	269	568	3333	6·30	1·250
41	1025	·5824	·1941	272	582	3417	6·15	1·220
42	1050	·5966	·1989	275	596	3500	6·00	1·191
43	1075	·6108	·2036	279	610	3583	5·86	1·163
44	1100	·6252	·2083	282	624	3667	5·73	1·137
45	1125	·6394	·2131	285	639	3750	5·60	1·111
46	1150	·6536	·2178	289	653	3833	5·48	1·087
47	1175	·6678	·2225	291	667	3917	5·36	1·064
48	1200	·6820	·2273	294	681	4000	5·25	1·042
49	1225	·6962	·2320	297	695	4083	5·14	1·021
50	1250	·7104	·2367	300	710	4167	5·04	1·000

A METHOD OF DUPLEX WORKING.

The method of duplex working described in the following paper can be worked successfully on land lines three or four hundred miles in length, provided the insulation is moderately good.

The idea occurred to me while making some experiments in connection with duplex telegraphy. I tried it with success under rather unfavourable conditions, but do not claim for it any superiority over existing methods.

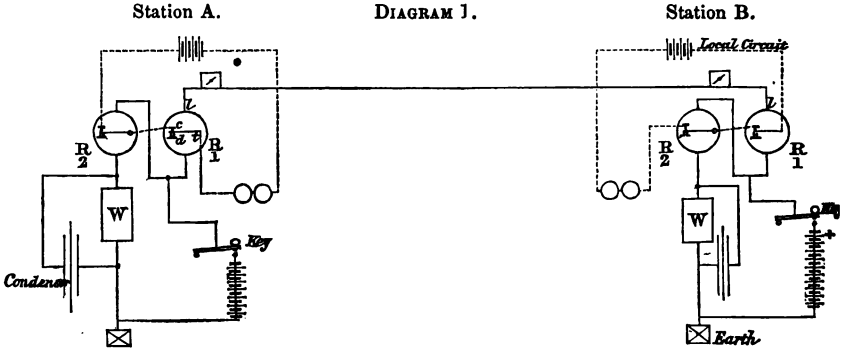
The distinctive feature of the system is that I use two relays instead of one.

My first experiments were made through large artificial resistances. I then worked from Kurrachee to Gwadur, a distance of 407 miles, and also from Kurrachee to Ormara and back, a circuit of 440 miles in length. Through 210 miles of land-line the working was still more satisfactory.

There was but one condenser available, and that at the Kurrachee end of the line; I therefore instructed Gwadur to use as a substitute the land-line between that place and Charbar, a length of about 100 miles. The absence of condensers of proper capacity rendered it difficult to exactly neutralise the return current, but, notwithstanding this, good results were obtained over the above-mentioned lengths.

The connections are shown in the accompanying diagrams :—

Station A. DIAGRAM 1. Station B.



During the passage of a current the tongue t of each Relay is attracted towards that side on which the positive current enters.

One of the relays R_1 (diagram 1) is in connection with the line, and the second relay R_2 makes earth through a resistance-box W, in which is unplugged a resistance approximating to that of the line. Absolute accuracy, however, in this respect is unnecessary. On pressing the key the current divides itself through the two relays, one portion passing to earth through the resistance-box, the remainder traversing the line relay towards the distant station. When the connections are arranged as in diagram 1, the outgoing current opens the local circuit in R_1 and closes it in R_2, but as these actions take place simultaneously the local circuit is never completed by the outgoing current.

The line relay R_1 is adjusted with the tongue t pressing so firmly against the local contact-point c that the outgoing current will only just draw it back to the agate-stop a; the left-hand relay R_2 is adjusted as if for ordinary working.

When receiving, the current from B, on arrival at station A, enters the line relay at l, drawing the tongue t still more firmly against the contact-point c; relay R_1 is consequently undisturbed by the passing of the current, which completes the local circuit by closing R_2, and thus causes the signal to be recorded on the tape.

It will thus be seen that, while sending, both relays are acted on without closing the local circuit, and, while receiving the current from B, has only to work one relay, that is R_2, as in ordinary single working.

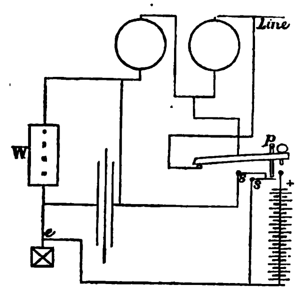
Now suppose the key at A depressed, relay R_2 is now permanently closed, while R_1 is open with the tongue resting very lightly against the agate-stop, but susceptible of being closed by any current arriving from B. Immediately the key at B is pressed the local circuit at A is completed by the closing of R_1, and the sender at B can continue to record signals by acting on R_1 with as little difficulty as when working the other relay; the depression of the key compels the incoming current to work the right-hand relay instead of the left.

Now supposing B to be in the middle of a signal when I press down my key, both relays are already closed, and my current assists still further in closing R_2, but is quite unable to open R_1

until B takes his current off; the signals from B therefore continue unbroken.

If while sending you record your own signals the outgoing current does not open the local circuit in R_1; if on the contrary you record reversed signals the left relay requires opening.

DIAGRAM 2.



In diagram 2 certain modifications are shown which have for their object the cutting out of circuit the branch resistance and the line relay when the key is in its normal position, the left relay being then the only resistance left in circuit at the receiving station. When the key is depressed the branch resistance and line relay are thrown into circuit, but short-circuited again immediately the key leaves the copper contact; the ebonite screw p is adjusted so as to break contact between the springs s s just before the battery is connected to the line.

This arrangement in no way interferes with the duplex working, and dispenses with the necessity of making any change in the connections if it is desired to resort to single working for a time.

H. C. MANCE.

INDIAN AND AMERICAN TELEGRAPHS.

In March, 1873, Mr. W. E. Ayrton read a paper, entitled "On some Points in connection with the Indian Telegraphs," before the Society of Telegraph Engineers. This paper is published in No. 5, vol. ii. of the Journal of the Society.

Mr. Ayrton first refers to the application of Mr. Schwendler's formula for reducing the apparent or observed resistance of either insulation or conductivity to its true value. Soon after the publication of Mr. Schwendler's formula I applied it in the measurement of insulation and conductivity of overland lines, but never found it to agree, or even approximate, to the result of a careful measurement, taken under circumstances where the insulation resistance was so large as not to affect the result, as measured in the ordinary manner; that is, to take the mean of the observed resistance with alternating poles of the battery.

Mr. Schwendler's formula is based upon the supposition that the insulation conductivities are uniformly distributed throughout the line, a condition of affairs that never exists in this country except in clear or cold weather, and when the escape of current by the insulators is so small as not to enter into the result.

In summer, long lines are often affected by local showers: it may be clear on some portions, and raining at others. During general rains, "north-east storms," humidity that affects insulators is very unevenly distributed—raining in some portions and only cloudy in others. The conductivity of insulation is never stationary under these circumstances, but as varying as the amount of rainfall at different intervals. I refer to the ordinary glass insulators of this country.

On very old wires, whose resistance is great through bad joints, conductivity is immensely improved by rain or dampness. So that, in my experience, it is idle to attempt the measurement of wire resistances or line conductivities, except by short sections or under favourable conditions of weather.

Mr. Ayrton says, "We have learned two very important facts," one of which is, "that the insulation of sections varies enormously

under the same climatic influences." If this be true, how do they arrive at the real conductivity of the wire? It also speaks badly for the insulators, provided we are to understand by the term "same climatic influence" that there is sensible leakage on any portion in clear weather.

A porous porcelain insulator conducts in clear weather; a glass insulator under same circumstances does not. By this is meant, when both are exposed to the sun's rays and not affected by moisture on the surface.

Mr. Ayrton enters into a calculation by which he is led to believe the consumption of battery material is increased, in his particular case, 60 per cent. by the introduction of the 1 per cent. of bad insulators. This calculation leads to erroneous results, inasmuch as it is based upon the supposition that there is no consumption when the circuit is open or the exterior resistance is infinite. Consumption of material is going on when the circuit is open, and this is particularly the case with the kind of battery he refers to as used in India, the Menotti.

In our country this battery without the intervening layer of sand or sawdust is most used. It is best adapted to our mode of working the closed circuit system. The size of the elements is sufficient to bring the resistance down to five and sometimes two Siemens' units per cell. If the battery is much worked this fact tends to keep the solutions separate. The more wires worked from the same battery, or the less the exterior resistance, the less the local action, and in this sense the more it is worked the greater the economy.

It answers admirably for working many lines from the same battery.

This battery is called the Callaud, and is essentially the same as used by the French at their central station in Paris.

Mr. Ayrton describes the manner of testing insulators at the factory and upon their arrival in Bombay, showing the great care bestowed upon the insulation.

Are the benefits derived commensurate with the care bestowed?

This question was put in another form by Mr. Preece, and the answer of Mr. Ayrton is, "In dry weather the insulation varied

from five millions a mile to over three hundred, in wet weather it was as low as half a million;" and that "the average was two to three millions in wet weather."

I infer that better results are obtained in this country with our poorest insulation. In measuring say 100 miles we seldom find it as low as one million per mile in rain. Leaving out the city portions, it will run as high as eight and seldom as low as four millions.

If we measure the insulation from this city to New York, the insulation of the same wire will measure twice as high when taken from the outskirts, and say six miles of city lines excluded as compared to the entire line, including city portion, where the measure is taken from the central station.

The insulation in cities is greatly reduced by smoke and gases of combustion which "short-circuits" the insulation, making the result appear below the average. Again, measuring from the country into a city, where the insulators are thus affected, the results appear above the average. Comparing the insulation of the lines in India, I should infer they could be improved by using the common insulator of this country.

As regards progress in insulation, are they to be compared with many lines in this country that average at least one hundred millions per mile in rain? We have thousands of miles of such lines in which I have a personal interest, and it would be out of place for me in this article to refer to them in detail.

Earth-plate.

The second "important fact" developed by the mode of testing in India was that the earth-plates had "a high resistance and were intensely polarised." "This was remedied by using large copper earth-plates with leading wires insulated from the ground."

Many people think copper better for ground-plates than iron in an electrical sense, but copper on account of its better conducting qualities possesses no advantage over iron, and zinc-coated iron is more economical and equally durable.

What advantage copper possesses over iron for ground-plates I

am unable to see. A ground-plate is simply a joint between two conductors, the wire and the earth, the latter specifically being a very poor conductor. If the sections of an iron and copper wire were joined, the iron being six times as large as the copper to ensure equal conductivity, the section of iron in contact with the copper would not conduct sufficiently to make a joint equal in conductivity with a section of either the iron or the copper wire. But if we enlarge the end of the copper so as to cover the section of the iron then the joint has a capacity for conduction equal to a section of either. We can, after understanding the nature of this joint, draw upon the imagination to form an idea how immensely an iron wire must be enlarged, and the extent of surface exposed, to fulfil the conditions of a joint with the earth. Supposing the earth in contact with the plate to be one-fourth saturated or contain one-fourth moisture, the plate would require at least ten thousand square feet of surface in contact with the earth to ensure a joint electrically perfect, or equal to a section of No. 8 iron wire.

With perfect joints the immense section of the earth, considered as a conductor, adds nothing to the resistance; or, in other words, if we make two perfect joints with the earth, there is nothing added to the resistance of the circuit.

The defects of ground-plates in connection with lightning-rods have been much discussed by the meteorological section of the Franklin Institute of this city latterly. It has been the custom to terminate lightning-rods in dry earth, often without any plate attached. It is the belief of those who have given the subject much attention that rods thus constructed are the cause of more harm than protection.

Every summer many valuable barns with their contents are destroyed, and in a majority of cases these barns are supplied with rods improperly connected with the earth. These rods draw the discharge from the clouds, and, there being insufficient means of getting from the rod to the earth, the charge flies to neighbouring conductors, often log-chains, ploughs, cart-tyres, or other metallic substances, igniting the combustible material with which these buildings are stored.

As an experiment, two plates, containing nine square feet of surface each, were buried fifty feet from each other, using the two plates and the earth as a portion of the circuit. They gave a resistance of 150 Siemens' units. Allowing a stream from a hydrant to flow directly over each plate, with holes in the ground above, so as to completely saturate, the resistance of these plates or rather the earth in contact was reduced to 32 units, or 16 units each, compared to an entire metallic circuit of No. 10 wire, 100 feet in length. By substituting 50 feet of wire in place of the earth, total resistance was reduced 32 units.

Instead of lightning-rods being connected to the earth in an electrical sense, they are, in truth, insulated from the earth.

People have often found it difficult to work short lines of telegraph in this country where the earth was used as a portion of the circuit, and connection was made or attempted by an ordinary ground-plate, exposing insufficient surface.

The writer was called to explain a difficulty of this kind about two years ago. A line, one mile and a half in length, connecting a factory with the proprietor's residence, was constructed with type-printing instruments. They failed to work, from insufficient current. The battery had been increased to 100 cells, the ground-plates overhauled and replaced with pure copper of increased surface, but all to no purpose, the apparent current was feeble.

The resistance of this line including the earth-connections was found to be 1600 units. Owing to the dryness of the soil the earth-plates made poor connection. Within a hundred yards of each terminal there happened to be a railway track; connection was made to this track, and the strength of current increased thereby to the extent that ten cells were ample to work the line.

A railway track, as the rails are joined in this country, makes a perfect ground even when the earth is dry, owing to the immense surface in contact.

A ground-plate, as usually constructed in this country, seldom interposes sufficient resistance to affect ordinary lines. The longer the wire or the greater the resistance, the less the effect in the proportion this resistance bears to the total resistance of the circuit.

In this sense also the polarization of the ground-plates is small, in proportion as the resistance of the line-wire and instruments is large. In the case of the India lines or circuits of 500 miles, I am unable to see how its effects are even appreciable. Insulating the wire leading to the plate to prevent it would do so, so far as this action is local, or confined within the limits of the plate and its connecting wire.

How this effect could now be shown on the instruments of the line, were the leading wires uninsulated, I am unable to comprehend.

Signalling.

The portion of Mr. Ayrton's paper that elicits most remark in this country is as follows: "To facilitate this reading by sound, the receiving signaller gives an acknowledgment by sending a dot at the end of every word, and the sending signaller continues repeating the word until he gets this acknowledgment."

By this method, the receiving "signaller" would be obliged to drop his pen to acknowledge every word as transmitted, or at least work under great disadvantages. We are unable to see how two good operators could by this method get more than twenty messages per hour over a line, these messages to average twenty words each, while by our method of closed circuit forty messages per hour is a moderate rate of speed. The sounder by the American plan responds to every touch of the sending operator. Any accident or change in the condition of the line involves a change of adjustment of his relay. If such change takes place either by increase or decrease of current the sending operator pauses for a reply from the receiving operator. On the circuits where much business is performed the operator sends for hours without a reply, yet he has assurance that every word is being correctly received. Especially is this the case when despatches are going in the other direction from the same station. The sending operator is assured of the correct transmission, otherwise he would be advised by the other wires. So it is not the custom in this country to stop sending merely for the purpose of ascertaining whether the despatches are correctly received. Whenever

the sending operator stops and closes the circuit, then the receiving operator replies or acknowledges.

By the duplex system, lately introduced, this method is increased. We may say the average is from 60 to 70 messages per hour on a single wire.

I would like to hear from Mr. Ayrton how this speed of transmission compares with that of the Indian lines.

Perhaps a more pertinent question would be, Do those interested in the Indian lines get as great a return for their expenditure in erecting and operating a No. 1 or No. 3 wire by their method as obtained in this country with lines of equal length, but of less than one-third the size, and the American method?

If I am able to judge fairly I should say lines in this country, costing half as much as the Indian lines, do twice or more than twice the business.

The methods of operating and maintaining the Indian lines are referred to in the light of progress or advancement in telegraphy—we are considering them in that sense from an American standpoint.

In the discussion following Mr. Ayrton's paper, Mr. Preece states "that sounders had been introduced almost entirely in America."

As early as 1860 nine-tenths of the instruments in the larger and more important stations were sounders, and now there is scarcely a Morse recorder to be found in any of these stations.

The recording instruments are used only to a very limited extent, in the way-stations of railway lines where the least business is performed. They are used by operators who are learners until they are able to read by sound.

In the year 1850 "Jemmy" Leonard, of Louisville, got the reputation of being the most expert "receiver" in the country, and entirely by ear. Since that time, or soon after, it became the ambition of operators to emulate Leonard, and receiving by sound from that date has been popular.

In 1852 I designed the first sounders used upon the lines of which I had charge. They are now made with from 4 to 8 units resistance, and usually worked by two cells Callaud battery.

In the years 1867-8 Mr. Varley made a report on the lines of the Western Union Company. In speaking of relays he says, "Keep down the resistance of everything is a golden rule in telegraphy." He found the relays in the New York office "to vary from 69 to 1182 ohms," and that "the 69 ohm relay is, where it should be, on a long line No. 2 Chicago;" and further, "The resistance should not exceed 130 ohms per relay, and it will pay you to have them all re-wound." Acting upon this advice the resistance of the relays was reduced.

For circuits of one hundred to two hundred miles they averaged about 50 units, and from 100 to 200 cells of Grove battery were required to work the lines.

Mr. Ayrton says, in speaking of relays, "We have not, however, used any relays having more than about 3,600 or 4,000 ohms resistance." I merely refer to these opinions to show that "doctors disagree," and that there is no question upon which we have such a diversity of opinions as upon the proper resistance of the relay. I would like much to see a relay made to fulfil the requirements as demonstrated by Eisenlohe, Dumoncel, and others.

The resistance of the relay, I will venture an opinion, has much less to do with the good working of lines, practically, than other conditions.

The Philadelphia and Reading Railroad work, a circuit of 140 miles with 40 relays, average resistance 100 units each. This line is used in connection with its traffic and the running of trains. It works well in all weathers, and would were the resistance of the relays twice as large or twice as small.

The Pennsylvania Central Railroad has three similar circuits, with relays averaging 150 units each, at intervals of about five miles; this resistance is larger than necessary, but the excess, whatever it may do, occasions no inconvenience.

With ordinary insulation, or say an insulation resistance of two million units per mile, these lines could not be worked by our American system of closed circuits, with a battery at each terminal and the currents from both flowing when the circuit is closed.

When the sender opens the line the receiver's battery still sends a current through his relay over the line and insulators, which is

greater or less as the insulation is good or bad; the affective or working margin is the difference between this "escape" and the whole line current. When the line current is small, owing to the resistance of wire and relays, this margin is small, and the "escape" must be kept at a minimum.

The wire in connection with all stations on the main trunk of the Philadelphia and Reading Railroad before referred to has a resistance, including relays, of over 8,000 units; with such a resistance the current passing over the line is unavoidably small, and to be able to work such circuit the current escaping by the insulators must be small in comparison to that passing through the line.

The Philadelphia and Reading Railroad Company makes a more extensive use of the telegraph than any other railway in this country; they employ ten wires on the main line, with branches in all directions, as soon as they enter the coal regions, 130 miles distant from Philadelphia.

I refer to the railways in this country because they are the only parties operating the telegraph upon anything approaching what is usually termed scientific principles; and further, all that Mr. Ayrton says of the railway telegraphs in India is of a negative character.

The foregoing are suggestions upon reading Mr. Ayrton's paper, and the remarks in connection therewith of the other Members of the Society.

The Chairman said that the Indian Telegraphs at one period were the "roughest thing possible." "He believed at the present time the Indian Telegraphs were amongst the most scientifically worked telegraphs in the world."

In a free and open discussion I should say it was more scientific than practical.

Real advancement in telegraphy is best shown by comparing the results obtained with the means employed.

In this connection it is proper to refer to the Automatic Telegraph Company, that operates a single wire extending from New York to Washington, a distance of about 300 miles, and doing a local business with stations at New York, Newark, Trenton, Philadelphia, Baltimore, and Washington.

In this circuit there are fifteen relays. The line is worked in one circuit; or, in other words, while transmission is going on between any two stations, the balance of the line is unemployed so far as transmission of the business of the other stations is concerned.

This Company does an average daily business of upwards of 700 paid despatches. They have secured this business simply by promptness and efficiency, in competition with the old Company, having its wires extended to every considerable town in the country, with hundreds of branch offices in the cities mentioned. Ninety per cent. of this business is done during the six active business hours of the day, that is, between 9 a.m. and 3 p.m.

The last annual message of the President, containing 12,000 words, was transmitted from Washington to New York in twenty-two minutes by this single wire. Is there anything in any other part of the world (including India) that can make so favourable a showing?

From 1845 to 1867 I was connected with the Telegraphs in this country, except in 1851, when I constructed the first line of telegraph in Mexico, connecting the City of Mexico with Vera Cruz.

In 1867 I visited Europe, and examined the different systems with much interest; and again last year, in the capacity of United States Commissioner to the Vienna Exposition. I was appointed to make a report upon Electricity and Telegraphy. This report is now in the possession of our Government. My chief purpose was to gather information of processes and methods of value and interest to our people. In the discharge of this duty I was afforded every facility at Brussels, Berlin, Vienna, Berne, Paris, and London.

On the Continent we find the Hughes printing instrument largely in use at all of the central stations. The speed of this instrument is given as twice that of the Morse, which is also used to a less extent. The telegraphic journals are now discussing very freely the duplex transmission system, which is more in use in this country than any other, and with great advantage. The capacity of a wire, however, is not and cannot be doubled by its use. If there were no alterations or adjustments in connection with the compensating resistances and condensers required, it might approach it, but time is necessarily lost in obtaining and preserving

the balance, more especially in wet and unfavourable weather. Under no circumstances is there as much performed in this country with four operators and one wire with the duplex system as is performed by two operators and one wire with the Hughes printer in Belgium, Germany, and France.

In the central station at Paris we see them working the Hughes instrument at full speed on the long circuits, Paris to Havre, Paris to Brest, Paris to Marseilles, Paris to Lyons; also on the International, Paris to Brussels, Paris to Berne, Paris to Berlin, Paris to London. When I see this instrument worked to its full capacity, or say twice ordinary Morse speed, upon those circuits, in rain or unfavourable weather, I am forced to make comparisons as to the economy and efficiency of their systems with the manner in which the business is performed in my own country and in England.

My report refers to the amount of business performed or handled in London and Paris, the number of operators required to perform the service in each of those central stations, and the number and kind of instruments used.

If the French can perform, and will perform, as much on one wire as the English on four wires of the same length and resistance, with proportionately less operators, and say one-twentieth the battery power (using one and the same battery for five or six lines), it cannot be denied that practically the French system of telegraphs is not to be despised, and that it might be substituted even in England or India with profit.

I could go more into details upon this subject, but I do not in any way wish to forestall an official report.

DAVID BROOKS.

Philadelphia, Aug. 21, 1874.

ABSTRACTS AND EXTRACTS.

"Comptes Rendus," *Tome LXXVIII.*

No. 9.

ON THE MODE OF PRODUCTION OF CERTAIN INDUCTION-CURRENTS.

By M. A. Gaiffe.

THE INFLUENCE OF ALBUMINOID SUBSTANCES UPON ELECTRO-CAPILLARY PHENOMENA.

By M. Onimus.

These papers do not admit of abstraction.

No. 12.

ON MAGNETISM.

The continuation of a Paper by M. Gaugain.

ON THE MAGNETISATION OF STEEL.

By M. Bouty.

Let us suppose that the circuit includes only a constant battery and the coil which is magnetised.

When a steel needle, recently tempered, is introduced slowly into the bobbin, it acquires, at the end of a time not exceeding that of the introduction, a total given magnetism. When we slowly extract the needle, it preserves a certain residual magnetism. The repetition of the passage increases the total magnetism and the residual magnetism. These tend, by repeated passages, towards a limit A, and the magnetic moment y at the end of x passages is very nearly represented by the empirical formula—

$$y = A - \frac{B}{x}$$

where A — B represents the residual magnetic moment after the first passage.

The curious increase which is here under consideration depends

essentially upon intermittence of the action of the current, since prolongation of the current will not produce it.

Three other processes can be employed to magnetise the needle in the bobbin :—

(1) Introducing the needle, establishing the circuit, and withdrawing the needle slowly. (Establishment).

(2) Introducing the needle slowly, the current passing, interrupting the current, and withdrawing the needle. (Interruption).

(3) Introducing the needle, establishing and interrupting the circuit, withdrawing the needle. (Disruptive discharge.)

The repetition of each of these processes results in an augmentation of the magnetic moment of the needle.

When the circuit consists of two coils, P and Q, the effect of the extra-currents renders the phenomena complex. Let us suppose the bobbin P much more powerful than Q. If the two bobbins are placed in sequence, it is observed that a needle magnetised by a large number of passages through the bobbin Q (to the corresponding limit) results in a quick increase by interruption of the current. The increase obtained in P under the same conditions is insignificant; the direct extra current of P is sensible in Q, that of Q in P.

The results obtained by placing the two bobbins in derivation, the one with the other, confirm this conclusion.

A condenser C placed in circuit upon a derivation without resistance, produces, when the derived circuit is interrupted, a true direct extra current in all the rest of the circuit, and similarly an inverse extra current when the current is re-established. In effect, the points of bifurcation, A and B, are at the same potential when the current passes, since the derivation is without resistance, and consequently the condenser is not charged ; the difference of the potential in A and B is equal to the electro-motive force of the battery P when the current is interrupted. The condenser discharges itself during the period of establishment of the circuit, and charges itself during the period of interruption which produces the extra-currents.

The analogy between a coil and a condenser led me to study magnetisation in the case where the circuit contains a coil and a condenser. This case is realised in a Ruhmkorff's coil. The condenser is there placed in a derivation without resistance, upon which are effected the interruptions.

The condenser I have employed is that of a Ruhmkorff coil in which

the explosive distance is 3 to 4 centimetres. Such a condenser comports itself from the point of view of production of extra currents, as a bobbin of negligeable power ; but there is presented, during interruption, a novel effect not present in the case of two bobbins. The direct extra-current of the bobbin charges the condenser very strongly, and this loses immediately afterwards its excess of charge, darting into the bobbin a current of contrary direction to that of the battery. Thus the magnetised needles in the bobbin may become partially demagnetised by a series of passages.

There can be substituted in the Ruhmkorff coil another arrangement, which consists in placing the bobbin in the derivation and making the interruptions upon the principal circuit. The results are absolutely the same. In the second case the demagnetisation observed is very feeble if the bobbin is weak, and as much stronger as the bobbin is more powerful. It is indeed a reflex of the bobbin upon itself.

When the circuit consists of two bobbins the slow introduction of a core of soft iron in one of the bobbins, or its extraction, is in effect appreciable upon the magnetism of a needle placed in the other bobbin.

But if the core of soft iron is introduced slowly and withdrawn smartly, the direct induced current augments the magnetic moment of the needle placed in the second bobbin. The repetition of the same operation tends to force the moment of the needle towards a limit which it approaches very rapidly. The formula—

$$y = A + B\,(1 - 2^{-ax}),$$

where A, B, and a are constants, appears to represent the magnetic moment y after x passages.

The results are the same when the circuit contains an electro-magnet, of which we quickly detach the armature.

The following facts have been observed with a bichromate battery, mounted for several days, and presenting consequently the phenomena of polarisation of the electrodes. With a single bobbin the establishment of the current increased the moment of a magnetised needle, often enormously : the current possesses therefore at the moment of completion of the circuit a much greater intensity than it evinces an instant afterwards. When the resistance of the helix is increased, the polarisation is diminished, and the peculiar effect of establishment tends to disappear.

As to the interruptions, they have no marked effect upon the needles, especially as the resistance of the bobbin is not very great. But in the

latter case, if we introduce into the bobbin a needle strongly magnetised, with its austral pole to the left of the principal current, we obtain always, by interruption, a diminution of the magnetic moment of the needle. The direct extra-current of the bobbin in its interior increased momentarily the polarisation of the battery, where the current of depolarisation very clearly follows that of the interruption; this current partially demagnetises the needles. We have then a case of reflex action analogous to that of the condenser of a Ruhmkorff coil, but incomparably feeble.

If the circuit contains two bottoms, the one A very strong, the other B very feeble, the direct extra-current of A and the reflex action proves the depolarisation succeeding itself in B and producing a very strange effect. The interruption of the circuit much augments the magnetic moment of a magnetised needle in B to a limit which corresponds to the passages ; but the same operation diminishes the magnetic moment of a needle strongly magnetised and placed in B with its austral pole to the left of the principal current.

We can see by this example that in certain cases we can analyse an instantaneous complex current by means of the magnetic action exercised by the current upon magnetised or non-magnetised steel needles.

No. 12—*continued.*

CALORIFIC EFFECTS OF MAGNETISM IN AN ELECTRO-MAGNET OF SEVERAL POLES.

By M. A. Cazin.

I have observed the calorific effects that accompany the disappearance of magnetism in the core of a rectilinear electro-magnet presenting several *consequent points*, and I have arrived at a very simple law.

"*When a rectilinear core of iron is magnetised by a series of identical bobbins, traversed by the current in alternately opposed directions, and these bobbins determine equal concamerations, the qualities of heat created in the core by the disappearance of the magnetism are inversely proportional to the squares of the number of concamerations.*"

The apparatus I have used is a kind of differential air-thermometer, of which the reservoirs are formed by cylinders of iron, 42 centimetres in length, 5 centimetres diameter, and about 2 m.m. depth. The bases of these cylinders are closed by plates of copper. A glass tube with an interior diameter of 2 m.m. bent to U-shape, and containing water, unites

the two cylinders; a manometer is employed to measure the difference of pressure H established between the two reservoirs when one of them is heated. This relative thermic effect is proportional to the difference of temperature of the two reservoirs.

To make an experiment, a non-continuous current is admitted to the magnetising bobbin. A counter registers the number n of interruptions; each minute is noted the difference of level in the manometer, and also the degree to which the primitive level is established when the current is suppressed. A graphic tracing of the results admits of ascertaining the total effect H produced during the action of the non-continuous current and of the correction h, due to the cooling action of the surrounding bodies. Dividing $H + h$ by the number n of interruptions, we have the effect of the disappearance of the magnetism at each rupture of the voltaic circuit. This quantity will be the relative measure of the heat created by the magnetism.

[Experiments are here described.]

I have also constructed an apparatus of precision, by the aid of which I can evaluate in calorics the observed thermic effect. I hope that this will permit me to determine the *magnetic equivalent of heat.*

No. 13.

ON AN ELECTRO-AUTOMOTOR WHISTLE FOR LOCOMOTIVES.

By MM. Lartigue and Forest.

This apparatus consists of a steam-whistle, in which the valve is manipulated by a lever, under the action of a spring. Ordinarily, the lever is maintained elevated by an electro-magnet, upon Hughes's system, and the valve is closed. Upon the passage in the coils of a current of given direction, the electro-magnet looses the lever and the whistle is sounded.

The apparatus, fixed upon a locomotive, is connected by an insulated wire to a metallic brush placed behind the fire-grate. This brush passes within a few centimetres of the way-plane. At a certain distance from a semaphore, or the visible signal, is placed, slightly above the way-plane, a small piece of wood bearing a metallic plate, that, when the signal is at "stop," forms the terminal of a source of electricity. The brush passing

on to this plate establishes a circuit through the detention magnet of the whistle, and the whistle is blown.

The arrangement has been extensively used in France, and successfully even at high speeds.

DEMONSTRATION THAT ELECTRIC INDUCTION DOES NOT TRAVERSE CONDUCTING MASSES.

By Prof. Volpicelli.

A mathematical demonstration of Faraday's experimental deduction. The paper is best consulted in the original.

No. 14.

ACTION OF THE ELECTRIC FLUID UPON GASES.

By M. Neyreneuf.

. . . . . No effect has been obtained with the induction spark, as remarked by M. du Moncel.

. . . . . Are the effects observed upon flames due to the simultaneous action of the two electricities, or to either acting separately? To solve this question it will suffice to interpose between the flame and the point a diaphragm impervious to the establishment of a gaseous current. With plates of glass, of sulphur, of gum-lac, the deflection is as energetic as before their interposition. A conducting plate communicating with earth was also tried. This last arrangement constitutes a simple demonstration of the propagation of induction in curved lines.

ON A NEW COUPLE, PREPARED SPECIALLY FOR THE APPLICATION OF CONTINUOUS CURRENTS TO THERAPEUTIC PURPOSES.

By M. J. Morin.

In a note presented to the Academy (*Comptes Rendus*, 24 June, 1872), I described a sulphate of copper couple, in which I sought completely to avoid the deposition of copper, neither upon the diaphragm nor upon the zinc itself. To avoid the bulky apparatus of former constructions I have devised a new couple, having a great analogy to that of Bunsen, although it is somewhat inferior in its electromotive force. The central carbon is surrounded with a chromic salt, representing as nearly as possible—less

the water—the chemical constitution of Jacobi's solution. This salt is dissolved by the water of the zinc bath. Some of these cells have been in use several months.

ON THE MEASURE OF THE ELECTROMOTIVE FORCE OF BATTERIES IN ABSOLUTE UNITS.

By M. A. Crova.

In reducing into absolute units the electromotive forces of several elements obtained by the method of opposition, we find numbers always superior to those we deduce either by Ohm's method or from the heat disengaged by the chemical reactions taking place in the element. We know that within certain limits the electromotive force depends upon the intensity of the current that traverses the circuit,* and that these variations have been attributed by several physicists to the polarisation of the negative plate. In order to ascertain the number we should finally adopt to represent the absolute electromotive force of an element I have employed the following method:—

Let h, h', h'', . . . be the interpolar resistances; i, i', i'', . . . the corresponding values of the intensity of the current produced by a given element; instead of taking h and i as variables, trace the curve, of which the abscissæ are the values of i and the ordinates the corresponding values of hi. We shall obtain a right line if the element is constant, and we shall have, in representing by y the values of hi,

$$hi = y = \mathrm{A} - ri,$$

the equation of a right line of which the ordinate at the origin represents the electromotive force, and of which the angular co-efficient is the resistance of the element.

If we obtain, by experiment, n values of h corresponding to an equal number of values of i, we shall have n points of the line sought, and, if it be a right line, we can deduce from its construction the mean values of A and of r.

In working with Daniell's and with Grove's elements I have observed that the line is a right line in nearly all its length, but it is slightly inclined in the neighbourhood of the axis y owing to an increase in the angle r. With single liquid elements, the right line is much more limited, and the curved portion acquires a greater development. I have continued, in these researches, to make use of units that I have adopted

* Marié-Davy. *Annales de Chimie et de Physique*, 3e Serie, tome XIX.

in my previous works,* that is, the resistance at zero of a column of mercury 1 metre in length and of 109· m.m. section, and an intensity of current that in one hour will decompose 9 milligrammes of water.

We will pass now to the numbers obtained in absolute units, by multiplying those obtained experimentally by the proper factors. The electromotive force of a Daniell (zinc, sulphate of zinc, copper, sulphate of copper), of which the plates have each 40 □ centimetres surface, is at the temperature of 11° equal to 43·1. The intensity varying from 1 to zero, the electromotive force increases from 43·1 to 43·9, the value given by Poggendorff.

The electromotive force of a Grove element of the same dimensions, and under the same conditions, becomes constant and equal to 75·0 when the intensity of the current is superior to the unit. The intensity varying between unity and zero, the electromotive force increases from 75·0 to 78·0, the value given by the method of opposition.

The resistance of a Daniell element varies from 5 to 15 metres, according to the concentration of the sulphate of zinc. That of Grove's element is about 1 metre. Now MM. Favre and Silbermann† have found that 1 gramme of zinc in substituting itself for the copper in $Cu. SO^4$ disengages 714 calories. The absolute electromotive force of the Daniell element will then be—

$$x = 714 \times 0{\cdot}000033858 \times 415{\cdot}41 \times 10^{10} = 11{\cdot}022 \times 10^{10},$$

0·000033858 representing in grammes the electro-chemical equivalent of zinc, and $415{\cdot}41 \times 10^{10}$ the mechanical equivalent in absolute units, of a calorie (gramme-degree). The electromotive force of a Daniell element under the conditions in which it is constant being 43·1, it is necessary to convert this into absolute units, to multiply by 0·2666, the value in Weber electro-magnetic units of my unit of intensity, and by the absolute resistance of the mercurial unit.

The mean of the results (0·9629 and 0·9564) obtained by the Committee of the British Association‡ is in effect $0{\cdot}9596 \times 10^{10}$. Weber has given 0·9749; § finally, the recent labours of M. Lorenz, || and those of M. Kohlrausch,¶ give respectively 0·9337 and $0{\cdot}9717 \times 10^{10}$.

* *Annales de Chimie et de Physique*, 3e Serie, tome LXVIII., et 4e Serie, tome IV.

† *Annales de Chimie et de Physique*, 3e Serie, tome XXXVII.

‡ *Reports of the British Association* for 1862–64.

§ WEBER. *Elektrodynamische Maasbestimmungen, insbesondere Widerstandmessungen.*

|| LORENZ. *Pogg. Ann.* Bd. CLXIX. (1873.)

¶ KOHLRAUSCH. *Pogg. Ann. Ergänzungs*, Bd. VI.

In the presence of results so divergent, I have calculated the co-efficient it is necessary to adopt to render the calculated electromotive force equal to that deduced from the heat of substitution. We have—

$$43{\cdot}1 \times 0{\cdot}2666x \times 10^{10} = 11{\cdot}022 \times 10^{10}, \text{ whence } x = 0{\cdot}95891,$$

a number which very nearly approaches 0·9596, the mean of the results of the British Association.

Bosscha had obtained, for the value of the electromotive force of a Daniell element, 10·258;* Thomson 10·79. These differences appear to me to indicate a value of the absolute unit of resistance more or less inexact.

The electromotive force of a Smee element, of which each plate has a surface of 9 ▭ centimetres, employed in vacuo (to avoid depolarisation of the negative plate by air), is comprised between a maximum 34·529 and a minimum 31·709.

Converting these numbers into absolute units by means of the factors 0·2666 and 0·95981, we find respectively 8·109 and 8·830 × 10^{10}.

Now MM. Favre and Silbermann have found that one gramme of zinc substituting itself for hydrogen in SO^4H disengages 567·9 calories. We deduce then for the Smee element the number 7·9885 × 10^{10}. Here the agreement is less exact than in the case of so-called *constant current* batteries. The result obtained, however, is conclusive.

No. 15.

AN APPARATUS FOR SIGNALLING AUTOMATICALLY THE PRESENCE NEAR A SHIP OF ICEBERGS.

By M. R. F. Michel.

The apparatus consists essentially of a metallic thermometer, inclosed in a suitable box, suspended or fixed to the sides of the ship. The thermometer is a bi-metallic helix; it carries a small lever, which moves to right or left as the temperature of the helix is raised or lowered. When the temperature is lowered, the lever comes into contact with a small metallic button, and closes the circuit of a battery and an alarm-bell.

The application of the apparatus depends upon the ascertained fact that the water around an iceberg, to an extended radius, is cooled several degrees.

* Bosscha. *Pogg. Ann.* Bd. CI.; *Annales de Chimie et de Physique*, 3e Serie, tome LXV.

No. 16.

A NEW THERMO-ELECTRO PIPE.

By M. Clamond.

. . . . . . I have adopted, in the construction of my couples, the alloy of zinc and antimony, and plates of iron for armatures. I have adopted the alloy of antimony and zinc because it is a good conductor of electricity, and because the temperature of its fusion-point renders its application easy. But I ought, in passing, to say that this fact is in opposition to generally received ideas.

We know that the alloy, zinc and antimony, possesses its thermo-electric property at its maximum of intensity, when it is composed according to the chemical equivalents of the two metals which constitute it. Now experiment has led me to reduce the tension of my bars in such a manner as to gain in quantity that which I lose in tension. Thus the model I have the honour to submit to the Academy deposits twenty grammes of copper per hour, and a similar model, constructed with bars of the same alloy of a higher tension, deposits only twelve grammes per hour. The resistance of the bar diminishes more quickly than its electro-motive force; in fact, the constant $\frac{E}{R}$ of the couple increases. It results from this that the most energetic bars are not those that constitute the most energetic pile.

I employ iron preferably to copper or to German silver, because these metals are attacked, dissolved by the alloy, and the armatures they form are rapidly put *hors de service*. Iron, on the contrary, resists well.

The arrangement of the apparatus is the following:—

The bars are arranged in a crown, and are coupled in tension. These crowns, composed each of ten bars, are superposed and separated from each other by rounds of asbestos.

The whole forms a cylinder, of which the interior is luted with asbestos, and heated by means of a perforated tube of refractory earth. The gas mixing with the air issues into the interior of the tube, and burns in the annular space between the tube and the bars. The extremities of the crowns are joined by bands of copper fixed to two plates. The crowns can be coupled in tension or in surface: the surface that can be obtained from each crown is seven square decimeters, which gives thirty-five square decimeters for the whole of the pile. There can thus

be obtained a mean deposit of twenty grammes per hour of copper of good quality.

The present model consumes 170 litres, that is to say, about five centimes worth, of gas per hour, and deposits twenty grammes of copper, the consumption of gas per kilogramme of copper deposited being 2fr. 50.

The couples that constitute the apparatus weigh 200 grammes, being equivalent to a Bunsen couple of eighteen centimetres high. The electromotive force of the apparatus is to a Bunsen couple as five is to three.

No. 17.

ON CHEMICAL DYNAMICS.*

By M. Becquerel.

The new memoir that I have the honour to present to the Academy upon Dynamic Chemistry has for its aim to ascertain not only the mode of evolution of the bodies that combine, but also the intensity of the forces or affinities in virtue of which these combinations are effected in at least their reciprocal relations.

It may be admitted that the effects of reduction and others which take place in capillary spaces enter into the domain of chemical dynamics since they are due to electro-capillary actions, by which the constituent parts of the bodies are separated, in executing certain evolutions, in order to form new combinations. This is the principle of which I seek to deduce the consequences.

Several methods are employed to determine electromotive forces, and consequently the relation between the affinities of the two solutions that react one upon another, and that of a solution upon a gas adhering to a metallic surface.† M. Ed. Becquerel has made a profound study of the means employed to determine electromotive forces. The method of which he has made use includes an electro-magnetic balance and resistance coils, with electrodes and couples of some size. But these conditions cannot be accepted when working with cracked tubes and

* Though not bearing an electrical title, this paper is collaterally too important to be passed over.—(*Trans.*)

† See two memoirs by my son Edmond; principally: *Annale du Conservatoire des Arts et Meteirs*, tome I. p. 257, and *Annales et de Chimie et de Physique*, tome LXVIII.

small plates, the fissures having only some thousandths of a millimetre opening ; if we replace the cracked tubes by porous diaphragms the mixture of the two solutions is too easily effected for them to have only simple contact during the period of the experiments. This method, which is especially applicable to the effects of constant couples of intensity, is very difficult of application when polarisation occurs in the circuit, such as in the case we have to study.

I have then preferred to make use of the method of experimentation termed "by opposition," which is the most simple and exact, employing a thermo-electric pile. By this method and with the necessary precautions we annul the counter-current due to the polarisation of the plates plunged in the solutions.

The experiments, the object of this memoir, are the results produced between the constituents of two solutions that react chemically one upon another. There is sought successively, with two electrodes in gold, the electromotive force of two solutions reacting upon each other, then with electrodes of water. The water-couples play an important part in these experiments ; they are formed by immersing in each of the solutions a cracked tube filled with distilled water, containing each a plate of gold or of platinum accordingly as the solution is alkaline or not. The results obtained are such that, if we work with solutions of neutral salts, there is not observed any electric effect ; in the case where there is combination then there appears an electromotive force. These results show that the combination of an acid with an alkali will not operate under the relation of the evolution of its constituent part, as the reaction of a solution of a neutral salt. We take for unity the hundredth of the electromotive force of a sulphate of cadmium couple, which is the most constant standard we have yet employed.

The following are the results obtained, and these are the means of a large number of results :—

1st couple (gold electrodes) .	$\left\{\begin{array}{l} SO^3\ 6HO \\ KO\ 6HO \end{array}\right\}$	182·5	
2nd couple (water electrodes) .	$\left\{\begin{array}{l} SO^3\ 6HO \\ KO\ 6HO \end{array}\right\}$	37·5	
3rd couple (gold electrodes) .	$\left\{\begin{array}{l} SO^3\ 6HO \\ \text{water} \\ KO\ 6HO \end{array}\right\}$	152·0	
4th couple (gold electrodes) .	$\left\{\begin{array}{l} SO^3\ 6HO \\ \text{water} \end{array}\right\}$	36·0	150
5th couple (gold electrodes) .	$\left\{\begin{array}{l} KO\ 6HO \\ \text{water} \end{array}\right\}$	114·0	

From these results we draw the following conclusions: In substracting from the electromotive force of the first couple the sum of the electromotive forces of the sum of 4 and 5 we should have zero, whilst we have a difference equal to 32 that should be equal to the electromotive force of the second which is 37. The difference can only be attributed to the nearly inevitable errors in experiments so delicate as those in question.

Let us now pass to the results obtained in the combination of the nitric acid with the potash :—

Mean electromotive forces.

$$\text{1st couple} \left\{\begin{matrix} NO_5 6HO \\ KO\ 6HO \end{matrix}\right\} 374$$

$$\text{2nd couple} \left\{\begin{matrix} NO^5 6HO \\ KO\ 6HO \end{matrix}\right\} 85$$

$$\text{3rd couple} \left\{\begin{matrix} NO^5 6HO \\ \text{water} \\ KO\ 6HO \end{matrix}\right\} 287$$

$$\left.\begin{matrix} \text{4th couple} \left\{\begin{matrix} NO_5 6HO \\ \text{water} \end{matrix}\right\} 172 \\ \text{5th couple} \left\{\begin{matrix} KO\ 6HO \\ \text{water} \end{matrix}\right\} 114 \end{matrix}\right\} 286$$

Making the same calculation as for the preceding experiments, that is to say, subtracting 286 from 374, we have a difference 88, which represents the electromotive force of the reaction of the anhydrous nitric acid upon the anhydrous potash. The electromotive force of the second couple should be equal to 88, but there is a difference of 3; this difference is within the limits of experimental error.

We see by these results that sulphuric acid or nitric acid in combining with the potash takes up the six equivalents of water. The combination is effected in the following manner. When one of the two acids combines with the potash containing six equivalents of water, the same phenomena result as are effected with mixtures of two solutions of neutral salts, that is to say, a small quantity of sulphuric acid acts upon and unites with a quantity of the water in union with the potash; similarly that a small portion of this acts upon a small portion of the acid, so that, for an excessively short time, these three bodies are *en présence;* and there results a combination of two hydrates and one other of the acid with the alkali not hydrated, which subsequently return to the two others a portion of their water to set about an equilibrium of hydratation. It is impossible to explain otherwise the evolutions which take place in the combination of a hydrated acid with a hydrated alkali.

In augmenting the quantities of water, the results appear to be the same. Experiments made with SO^3, 12HO, and KO, 12HO have given sensibly similar results. We may therefore conclude that in the reaction of an acid solution upon an alkaline solution, the one and the other hydrated, there are produced electric effects resulting from three different combinations, and we may suppose them to operate in the mode indicated.

ON THE ELEMENTARY LAW OF ELECTRO-DYNAMIC ACTION.

BY M. MOUTIER.

Ampère has explained electro-dynamic phenomena by means of an elementary law, which rests upon the consideration of three cases of equilibrium. M. Bertrand has recently reduced the number of fundamental experiments to two.* The author of the *Theorie des Phénomènes Electro-dynamiques* has thus set aside all hypothesis relative to the intimate nature of the phenomena; but, under the influence of the discoveries of Fresnel, he was pre-occupied with analogies between light and electricity. In these last few years M. de Colnet d'Huart and M. Renard have correlated electro-dynamic phenomena to the theory of elasticity, and have recurred by different means to Ampère's formula.

In the memoir I have the honour to submit to the judgment of the Academy I have taken for the starting-point the considerations laid down by Ampère, and I have sought to formulate them from a mechanical point of view, regarding electricity as the result of a vibratory movement of the ether. In following this order of ideas I have already sought the explanation of some electric phenomena;† in this work I occupy my attention with electro-dynamic actions.

I. If we consider the electric current as a movement transmitted by the ether of the conductor, the intensity of the current is then measured by the quantity of movement of the ether with relation to the unit of length of the conductor.

II. Let us consider two current elements ds and ds' parallel to each other, and perpendicular to a right line joining them. Let i and i' be the intensities of the currents, r their distance.

* *Comptes Rendus*, tome LXXV. p. 733.

† *Comptes Rendus*, tome LXIII. p. 299.

At a point of one of the conductors ds the ether is driven at two velocities; one is due to the current ds itself, the other is derived from the current ds'. Admitting that the movement of the ether is propagated around each conductor traversed by a current according to the same law as with the theory of undulations, we may determine the relative velocity of the ether at any point of the conductor. The other exercises on this point a pressure proportional to the square of its relative velocity. Analysis of these pressures show that they exert between the two elementary currents a reciprocal action, directed according to the line of junction, attractive or repulsive, as the currents are the same or of contrary direction, and have for their expression

$$2 \frac{i\,ds\,i'\,ds'}{r^2}.$$

III. Let us consider, however, the case where two current elements are situated, the one on the prolongment of the other. We may assimilate the phenomena occurring in one of these conductors to the movement of a fluid which possesses velocities equal to the relative velocities of the ether at each point of the conductor. Applying then Bernoulli's theorem, the difference of pressure exerted at the extremities of an elementary conductor correspond to a reciprocal action, directed as the line joining the elements, attractive or repulsive, as the currents are of the contrary or of the same direction, and having for expressions,

$$\frac{i\,d\,s \quad i'\,d\,s'}{r^2}.$$

IV. It is easy to pass to the case of two elementary currents situated in such a manner as would arise from resolving two currents. Let θ and θ' be the angles of $d\,s$ and of $d\,s'$ with the line of junction of the two elements; ϵ the angle of the planes of the line of junction and of each of the elements. To ascertain the action of two elementary currents it is sufficient to examine the following mutual reactions:—

1. The action of two parallel currents perpendicular to the line of junction. This action, according to what precedes, has for its value—

$$f = 2\frac{i\,d\,s \;\; i'\,d\,s'}{r\,2} \sin\theta \sin\theta' \cos\epsilon.$$

2. The action of two currents placed in right line. This action has for its value—

$$f' = \frac{i\,d \quad i'\,d\,s'}{r^2} \cos\theta \cos\theta'.$$

3. The actions of other currents arising from the decomposition of $d\,s$ and $d\,s'$. These actions are nil in the preceding theory.

The mutual action of two currents is then—

$$F = f - f'.$$

We find thus the formula given by Ampère multiplied by 2; and we know that it is necessary to multiply this formula by 2 in order to give coincidence to the intensities of the electro-dynamic and the electro-magnetic current.

V. The work that results from the displacement of two conductors traversed by currents of constant intensity may, as is known, be deduced from Ampère's formula.

Geometric considerations show that, in the case of closed circuits, the elementary work between the two currents $d\,\tau$ depends only on the forces f'. It has for its expression—

$$d\,\tau = \quad - i\,i'\,d\,W,$$

where W represents the function that M. Helmholtz terms the *relative potential* to the action of two currents, when the intensities are equal to unity.

VI. Ampère's law does not apply to phenemona of induction. There are two cases to distinguish, either as induction is produced by change of intensity or by movement of the conductors:—

1. Let there be two closed circuits s and s' of which the intensities are i and i'. If the intensity of the current s' increases and becomes $i' + d\,i'$, the relative velocity of the ether in the conductor s shows a variation that corresponds to an induction-current in this conductor, and the demi-variation of the force of the ether of this conductor has for its value—

$$W\,i\;d\,i'.$$

We thus find the expression for the work relative to this phenomena of induction.

2. If, on the contrary, the intensity of the current s' remains constant, the conductor s' approaching s, there also results a variation of velocity in the ether of the conductor s; this change of velocity corresponds to an induction-current in this conductor, and the demi-variation of the force of the ether of the conductor has for its value—

$$i\,i'\,d\,W.$$

We thus obtain a known expression.

No. 18.

ON THE DEPTH OF A MAGNETISED STRATUM IN A STEEL BAR.

By M. Jamin.

. . . . . The quantity of magnetism developed in a stratum of which the depth is dy is proportional to the force, and equal to

$$\frac{F\,dy}{a^{y}}.$$

The definite integral for a depth y will be

$$m = M\left(1 - \frac{1}{a^{y}}\right),$$

and will represent the quantity of magnetism contained in a depth y. [M and a are taken equal to 26 and to 1·5] . . . .

No. 19.

ON THE DETERMINATION OF SIMPLE SUBSTANCES BY THE ACTION OF BATTERY-CURRENTS IN THE VOLTAMETER.

By M. E. Martin.

The author desires to show that, in the decomposition of water by a current, (1) the two electricities are not forces, but imponderable substances, having strong and different chemical affinities, and that they excite on the elements of water a double chemical action, transforming them into gases. (2). That water does not contain, as is supposed, two gases condensed, which we have merely to dissociate in order to reproduce, but that the simple bodies H and O are its elements. (3). That the two gases whose combination produces water are compound bodies formed by the chemical union of oxygen, a simple body, to positive electricity, and by the union of simple hydrogen to negative electricity; the combination of these gases giving two binary substances, water and caloric. (4). That the currents of the battery do not traverse the acidulated liquids of the voltameter, and do not result in the transport of elements, but the two electricities arriving at the electrodes unite with the elements of water, transforming them into hydrogen gas at the

negative pole and oxygen at the positive. Negative electricity may be distinguished as *electrile* with symbol *El*, and positive electricity as *etherile*, with symbol *Et*.

No. 22.

ON THE MEAN SECTION, POLAR SURFACES, AND ARMATURES OF MAGNETS.

By M. Jamin.

The author enunciates the following propositions :—

1. The number of elementary magnetic fillets, and consequently the quantity of magnetism, that a magnet can contain, depends only upon the mean section.

2. The distribution of the intensities is regulated by the form and by the extent of the exterior surfaces of the magnet.

3. If these surfaces diminish, the tension increases to the limit that the elemental poles can give it effect, and a portion of the two contrary magnetisms disappears in order to produce a natural condition.

These ideas the author confirms by a series of experiments, in the course of which he arrives at the following conclusions: If the surfaces change, and the mean sections remain the same, the quantity of magnetism is constant.

The distribution of the magnetism is regulated by the surface, and as this diminishes the tension increases.

If the mean sections be varied, the quantity of magnetism is proportional to these sections.

When the surface is insufficient, the total magnetism furnished by the mean section cannot have effect, and it reacts upon and reduces itself.

The total magnetism furnished by the mean section may be developed and conserved upon a plate of which the surfaces are insufficient, under the condition of supplying the deficiency of surface by iron armatures of considerable size.

No. 22—*continued.*

ON ELECTRIC CHRONOGRAPHS.

By M. Marcel Deprez.

In my preceding communication I announced that, having tried the different processes of electric registration, I found the induction spark possessed such inconveniences as stultified the sole advantage to be derived from its use—instantaneity. I thus have directed my attention to the use of electro-magnets.

But the data important to their application to chronographs are completely hidden. The carrying force, the effect of the armature and amount of attraction, the time during which the current traverses the coils of the electro-magnet—all these are elements of essential importance. These considerations led to the use of electro-magnets, not as registrars, but as a means of detention. The most simple arrangement consists in fixing the armature to a very short axis, to which again is attached the style, brought into contact with the revolving cylinder. A caoutchouc thread attached to a small armature itself, developes an effort of contrary sign and of nearly equal power to the magnetic attraction, so that by regulation the armature may be held against the magnet by a strength as feeble as we please. When the circuit is interrupted, the armature becomes free to follow the effects of contraction of the caoutchouc thread.

If we designate by f the tractive effort of the caoutchouc; f' the effort developed by the caoutchouc; l the length of the lever-arm to which it is attached; L the length of the pen; p the weight of the moving system; ρ^2 the square of the radius of gyration of this system with relation to the axis of rotation; g the weight; γ the linear acceleration at the extremity of the pen; we have $\gamma = g\frac{f l L}{p \rho^2}$.

This formula is exact only under the hypothesis that the magnetic force will be completely dissipated at breakage of contact.

If we suppose f constant, and that it be required to know the velocity v at the extremity of the pen, we shall have $v = \sqrt{2g\frac{f l L \lambda}{p\rho^2}}$.

It is clear that, for a given value of λ, this velocity is proportional to the square root of the moment of the force f, and inversely proportional to the square root of the moment of inertia of the moving system.

To attain a high velocity it is necessary to concentrate on a very small armature a very great magnetic force.

The following apparatus solves the difficulty. It consists of a pen fixed to a small axis moving on two pivots, and to which is soldered a lever-arm, at the extremity of which is a small plate of parchment paper. The weight of this paper is about ten milligrammes, and the resistance to traction about one kilogramme. It is fixed between the armature and the pole of the electro-magnet. A caoutchouc thread exerts, by the intervention of the lever, a power of 400 to 500 grammes, or even one kilogramme. Thus arranged, the apparatus forms the first type of my registers, and I have employed it in the measurement of the pressure of powder.

No. 24.

ON PHENOMENA OF STATIC INDUCTION PRODUCED WITH THE RUHMKORFF COIL.

By M. E. Bichat.

The most suitable apparatus for the transformation of dynamic electricity into static electricity is, without doubt, the Ruhmkorff induction coil; reciprocally, the same coil should be the apparatus best suited to the inverse transformation of static into dynamic electricity. If there be passed a series of sparks of static electricity, produced by a Holtz machine, through the fine wire of a coil, in the thicker wire induced currents will arise, and these currents are remarkable amongst other induction currents produced by static electricity for the extreme facility with which they decompose water and salts. Theoretically there should result two induced currents of equal quantity, and consequently two equal quantities of detonating gas, at the two electrodes of the voltameter. Now this is not the case. For, the experiment being made with the largest of the induction coils constructed by M. Ruhmkorff, on one side is disengaged hydrogen and on the other oxygen nearly pure. If instead of decomposing water sulphate of copper is decomposed, at only one of the electrodes is copper present, and always so directed as indicating decomposition from an inverse current. It appears, then, according to this experiment, that we have a unique current, which acts in contradiction to the known facts of induction. This apparent contradiction I seek to explain.

It is natural to ask, What is the nature of the influence of the bundle of iron wires in the Ruhmkorff coil, which contribute in so powerful a manner to the transformation of dynamic into static electricity? To ascertain this a small bobbin was procured, in which the iron had not been fixed. Here is a *resumé* of the observations:—

When the bobbin is empty the current is always feeble, but perfectly appreciable, and it is inverse when the bar of iron is introduced; the deflection which corresponds to an inverse current is much increased, and it becomes very considerable if the bar of iron is replaced by a bundle of iron wires.

It is to be remarked that the disengagement of gas produced upon the platinum wires of the voltameter when the coil is empty is very feeble and insignificant, becoming perfectly appreciable after the introduction of the soft iron; so that the gaseous disengagement, in the experiment made with the large coil, will be nearly entirely attributable to the influence of the soft iron.

If care be taken to examine the conditions of the experiment, it is easy however to explain these phenomena. Let us take the experiments on the polarisation of the electrodes, and suppose the bobbin empty. It is especially necessary to remark that the current from the Holtz machine is obliged to traverse a very long circuit of very small diameter, which constitutes a considerable resistance. The two electricities—positive and negative—accumulate little by little upon the terminals of the excitator, and when the tension is sufficient a spark will pass. We may express this fact more simply by saying that the spark commences slowly and ends on the contrary abruptly. There then result two induced currents, equal in quantity but very unequal in tension. The tension of the direct current which corresponds to the rupture is enormous compared with that of the inverse current. The inverse current that produces the former arrives at the voltameter and decomposes the water. This decomposition, which is slowly produced, results in the deposit upon the platinum wires of a very large quantity of microscopic gaseous bubbles that do not disengage themselves, and consequently are eminently suited to the production of currents of polarisation.

The direct current that subsequently arrives also decomposes water, but the decomposition is effected more rapidly. The bubbles of gas are larger, less adherent to the platinum wires, and immediately disengage themselves, producing only a feeble polarisation, completely incapable of destroying that which has been produced by the inverse current.

The apparent production of a unique inverse current is then due to the difference of tension of the two induced currents. If this is true, it remains to be seen whether the galvanometric deflection diminishes if by known means we diminish the tension of the direct current. This can be effected by the aid of diaphragms. If there is introduced into the empty coil a continuous copper tube the galvanometric deflection diminishes immediately. It is constant, on the contrary, if before the interposition of the diaphragm the continuous tube is replaced by a split tube.

Let us suppose, however, that a bar of soft iron is introduced into the coil. The phenomena produced when the coil was empty still occur; but at the same time the magnetisation of the soft iron gives rise to other induced currents which have effect upon the former. In order to comprehend what occurs it is necessary to remember that, of the two induced currents, direct and inverse, it is that magnetises more strongly which is possessed of the greatest tension—that is to say, the direct current. We may thus say that the magnetisation produced by the first spark is direct. On the other hand we know that there is always a certain time taken to demagnetise a piece of soft iron. The result is, that the sparks of the machine succeeding each other with rapidity, the magnetisation produced by the first spark will not be completely destroyed when the second spark arrives. This second spark aids the first, and increases the magnetisation of the soft iron ; it is also the same in continuance. We have then a direct magnetism which increases. This increasing magnetism should produce an inverse current, an opinion that experiment confirms.

As to the considerable augmentation of the effects produced by the inverse current when the bar is replaced by a bundle of soft iron wires, this is easily explained if, in the phenomena of static induction, as well as in the phenomena of induction, the interposition of diaphragms has the effect of diminishing the tension. The demonstration of this fact is very simple.

En resumé,—if, in the large wire of a Ruhmkorff coil, we pass a battery current, successively interrupted and re-established, we obtain in the fine wire two induced currents of contrary directions, and for a certain explosible distance it *appears* that there is only one current. This current is *direct*, and the sparks produced by this current have every appearance of sparks of static electricity.

Reciprocally, if in the fine wire there are passed a series of sparks of static electricity, there are obtained in the thicker wire currents analogous

to those furnished by the battery, and, studying these currents by means of the voltameter, it *appears* that there is only one current, and this current is *inverse.*

By reversing the machine we at the same time reverse the direction of the induced current.

No. 26.

ON ELECTRO-STATIC PHENOMENA IN BATTERIES.

By M. Alfred Angot.

We know that the fundamental property of the pile is, that between each element there exists a constant difference of potential a; this difference varies only with the nature of the substances in contact and their temperature, and is completely independent of their extent and peculiar electrical condition. This sole property is sufficient to establish the theory of electro-static phenomena in relation to batteries.

Let us suppose that we put a conducting body of electric capacity C in communication with one of the poles at the potential V of the battery, which we will suppose insulated. The potential decreases to v on this pole; but it is lowered at the same time and in the same quantity through all the battery, for the difference of potential of a couple must be constant, and independent of the absolute value of this potential.

In this way, as the potential at any point of a battery varies from one couple to the next, the contact of a conducting body produces in all points of the battery an equal "fall" of potential, exactly as on a conductor. Inversely, any electric charge which should raise from the potential v any point in a battery would produce the same elevation of potential at all points.

We may then speak of the electric capacity of a battery as of that of a conducting body; that capacity being the quantity of electricity necessary to elevate from unity the potential of any point of a battery.

The introduction of this notion of capacity of the battery, and the definition of this capacity, identical with that of a conducting body, admits the resolution of the problem of the dispersion of electricity

between a battery and a conductor, precisely in the same manner and by the same equations as the dispersion between two conductors.

As to the value of the capacity of the battery, from the moment we treat it as a conductor, we see no reason *à priori* that it should be different from the capacity of a conducting body of the same dimensions. This may be demonstrated directly by calculation; but I have given experimental verification in the following manner:

Let P be the capacity of the pile defined, as has been said; C that of a cylindrical conductor of the same dimensions; and E that of a body with which these are put into communication.

The potential at the terminal of the insulated battery being V, and falling to v by contact with the body E, the latter takes a charge M, so that we have

$$M_1 = (V - v)\, E = P v\,;$$

whence we deduce

$$v = V\frac{E}{P + E} \text{ and } M_1 = V\frac{PE}{P + E}.$$

If, on the other hand, we put into communication with the same body E the cylinder C at the potential V, the potential is then lowered to v', and the cylinder takes a charge M_1', so that we have

$$M_1' = (V - v')\, E = Cv',$$

whence

$$v' = V\frac{E}{C + E} \text{ and } M_1' = V\frac{CE}{C + E}.$$

If experiment gives $M_1 = M_1'$, it may be concluded that $C = P$, that is to say, the capacity of a battery, as previously defined, is expressed by the same number as that of a conducting cylinder of the same dimensions.

The experiments were made in the following manner:—

A Volta's column was taken, supported by insulated supports, of which the inferior pole had been put for an instant in communication with earth. The potential at the superior pole was then represented by the number n of the elements of the pile. This pole was then put in communication with an electrometer. The deflection δ_1 of the electrometer was proportional to its charge M_1, so that the quotient $\frac{\delta_1}{n}$ repre-

sented the value of the expression $\frac{M_1}{V} = \frac{PE}{P+E}$, E being the capacity of the electrometer. We thus obtained the following numbers:—

No. of elements.	Height of battery, *c.m.*	Deflection δ_1.	$\frac{\delta_1}{n}$.
10	6·15	1·3	13·0
20	12·45	3·8	19·0
30	18·90	6·6	22·0
40	21·40	9·35	23·4
50	25·00	10·35	25·9
60	38·40	19·0	31·6

Again, a conducting cylinder, of base equal to that of the pile, and of variable height, was put in communication with a Volta's pile of 100 elements, of which the potential was equal to 100 with the electrometer; also with the last the deflection was proportional to the charge M_1'. Experiment gave the following numbers:—

Height of cylinder, *c.m.*	δ_1'.
5·9	12·2
11·8	18·0
23·6	24·1
40·0	30·6

By graphic interpolation we may adduce from these numbers those related to cylinders having the same height as the pile at the different stages quoted above. We find thus:—

Height of cylinder, *c.m.*	δ_1'.
6·15	13·2
12·45	18·5
18·90	22·0
21·40	23·0
25·00	24·8
38·40	30·0

The agreement between these numbers, and the values of $\frac{\delta_1}{n}$ recorded above, proves the theorem advanced.

"COMPTES RENDUS," *Tome LXXIX.*

No. 1.

RESEARCHES ON ELECTRIC TRANSMISSION BY LIGNEOUS SUBSTANCES.

By the COUNT DU MONCEL.

For some time I have observed that ligneous substances transmit electric currents, and that it is possible to denote upon a sensitive galvanometer the presence of these currents under subservient conditions. I wished to assure myself whether, in this relative conductibility, the ligneous matter of the substance itself conducted, or whether the conduction was not due to the humidity with which these bodies are more or less impregnated, even when reputed *dry*. If this explanation were alone the true one, galvanometric experiment would furnish an easy means of measuring, not only the more or less humid state of wood, but the hygrometric properties of different kinds of wood, an important result in the matter of choosing wood for the construction of electrical apparatus of precision.

The experimental means were necessarily delicate, for a host of causes might intervene and furnish false results. Thus the degree of pressure upon the metallic plates in communication with the wood, its more or less perfect insulation from earth and from the walls of the apartment in which experiment was made, the size of the surface put in communication with the metallic circuit, the mass of the wood, the more or less humid condition of the air of the experimental cabinet, accidental contact of the fingers on the wires communicating with the galvanometer, and the insulation of these wires—these are among the causes modifying the results obtained. Even the energy of the currents transmitted had a peculiar and really curious influence. Thus, with my galvanometer of 36,000 turns, a current which, from one cause or other, was reduced from 9° to 7½°. and maintained indefinitely at the latter deflection as long as connection continued with the battery and galvanometer, would not yield this deflection when the galvanometer after returning to zero from breakage of the circuit was again traversed by the current. We may suppose that, as with vibrations of the pendulum, the force necessary to maintain electric movement already begun, was insufficient to start that move-

ment.* Under these conditions, indeed, my galvanometer, having accomplished an imperccpible movement, remained at zero, whilst influenced by a current which had maintained a deflection of $7\frac{1}{2}°$ for a considerable time. If the current were a little stronger, 9° for example, in place of $7\frac{1}{2}°$, this current passed as ordinarily.

It is easy to comprehend, after these experiments, that it is very difficult to affirm in all cases that the absence of galvanometric deflection indicates always the *complete absence* of currents, even supposing the galvanometer sufficiently sensitive to reveal them.

The experiments I have undertaken to solve the problem proposed were arranged in the following manner.

I obtained prisms of different kinds of wood, the prisms being 10 c.m. long by 2 c.m. in width and 1 c.m. in depth. I measured their conducting power by placing them between the two disjointed extremities of a circuit terminated by two plates of platinum, and I strongly pressed these plates upon the two extremities of the small plates of wood by means of bronze clamps. I caused to pass through this circuit, in which I included my galvanometer, the current from six sand bichromatic elements, and I noted the deflection at the end of five minutes from the closing of the circuit. When this first operation is completed, I insert the samples of the woods I wish to study for half an hour, and for two hours, and at each period I measure their conductive power. Subsequently, I allow them to remain with free access of air during the night before I recommence my measures. Finally, I place them in a closed vessel saturated with humidity by means of a syphon bearing water to the base of the vessel. I note the degree of humidity and I measure again the conducting power. The contact surfaces of the platinum plates do not exceed 6 □ c.m., and the interval between the plates is 6 c.m. Every precaution was taken to avoid the perturbations of which I have previously spoken.

I will not now give the figures I have obtained; I will indicate only the results:

1. A small prism of oak, of the dimensions mentioned, regarded by the cabinet-maker as being very dry, furnished, when it was brought me, a deflection of 55° with the battery I have described.

2. This small prism, when it had been dried for two hours in a stove, gave not the least deflection when again submitted to the test.

* We know that a pendulous weight in repose necessitates a great primary force to put it in motion; but that at a given period of its oscillation an exceedingly minute force suffices to maintain the motion indefinitely.

3. Preserved in a sunny chamber for several hours its conductivity was not increased.

4. Exposed to the air during a dry July night, it gave in the morning a deflection of 13°.

5. A table of large size, fastened it is true to a wall, but dry for more than six years, allowed the battery current to pass through a length of 2 metres, so that a deflection of 9° was obtained, and this deflection was increased to 12° when the length of the table interposed in the circuit was reduced to 50 c.m.

6. The degree of pressure of the plate of platinum against the wood has so much influence upon the intensity of the current transmitted, that we had 12° with a maximum pressure, falling to zero with the pressure of the plate's own weight, and to only 5° with a feeble pressure.

It results from these experiments that it is to the aspired humidity through its pores that wood owes its relative conductivity, and that this conductivity is proportionate to the degree of pressure upon the plates of communication with the circuit.

No. 2.

FURTHER RESEARCHES UPON THE ELECTRIC CONDUCTIVITY OF LIGNEOUS SUBSTANCES.

By the COUNT DU MONCEL.

In my last communication I have shown that the relative conductibility of wood is to be principally attributed to the greater or less humidity with which it is always more or less impregnated, and that, consequently, the different kinds of wood should have their conducting power proportionate to their hygrometric property. Before studying the conducting powers of different woods, I wished to ascertain in what proportions one kind of ligneous substance can absorb the humidity of the atmosphere at different periods of the day, and I arranged a piece of oak in such a manner as to constitute a hygrometer, observing at the same time the position of a hair-hygrometer, a rermometer, and the state of the sky.

The piece of oak, which was a small prism of 10 c.m. length, and of 2 c.m. depth and width, was connected to the galvanometer circuit and battery by gutta-percha-covered wires. Four plates of platinum were strongly pressed to the extremities of the prism by means of clamps; and the whole was suspended by the interocation of gutta-percha supports,

and cords of the same material, before the laboratory window. At mid-day the system received the sun's rays. From this, the alternation of humidity and dryness was more marked. The following is a *resumé* of experiments made at three-hourly periods during five consecutive days :—

	Conductibility of the wood.	Hygrometer.	Thermometer.
6 a.m.	16·9°	45·9°	18·4°
9 a.m.	15·0	36·7	21·1
Noon	12·1	24·9	24·2
3 p.m.	9·9	21·2	25·3
6 p.m.	7·5	28·9	22·5
9 p.m.	8·6	42·4	19·6
Midnight	10·8	48·9	17·4
3 a.m.	13·9	50·0	16·2

For a perfectly fine day these values, in twenty-four hours, were :—

	Wood conductibility.	Hygrometer.	Thermometer.
6 p.m.	9·0°	34·0°	22·0°
9 p.m.	11·0	49·0	18·0
Midnight	15·0	51·5	17·5
3 a.m.	18·0	50·0	16·2
6 a.m.	22·0	51·5	16·0
9 a.m.	19·0	32·0	22·5
Mid-day	13·5	19·0	25·0
3 p.m.	14·0	17·5	26·0

It is evident, according to this table, that the conductibility of the wood follows variations that affect the hydrometer, but that these variations are slower in the one case than in the other, because the maxima and the minima of conductibility are attained some time after the corresponding humidity of the air. It is towards 6 a.m. that the conductibility of the wood is greatest, and towards 6 p.m. that it attains its minimum value, while the hygrometric maxima and minima are at 3 a.m. and at 3 p.m. This effect is to be easily understood if we reflect that the humidity of the night, penetrating further into the wood, collects a greater quantity of water, which not only increases the conductibility, but preserves this for some time, that is to say, until its evaporation by the sun.

Temperature also exercises a peculiar action during the drying, since its increase gives increase to the conductibility of the absorbed liquid, and there results that, if there is a small difference between the dryings produced when the temperature rises from one degree to a higher degree there is thus an increase in the galvanometric deflection, instead of a decrease. This is precisely what takes place with the experiments furnishing the results given in the second table. Thus, the temperament being at mid-day 25° and at 3 o'clock 26°, the intensity of the transmitted current was at mid-day 13·5°, and at 3 o'clock 14°.

The experiments with the different kinds of wood have been repeated four times. The following are the results obtained :—

Wood.*	After 2 hours in moist cabinet.	After 2 hours in a stove.	At the moment of receipt from the joiner.	After 5 hours in moist cabinet.	After 2 hours again in the stove and 15 hours in moist cabinet.
Black Ebony (Diopyros ebenum) ...	75°	0	86°	80°	14°
Common Box	22	5° 0	76	35	17
Acacia	14	0	55	18·5	10
Poplar	27	0	85	44	17
Willow	10	5 0	50	21	15
Linden	10	0	87	24	43
Chestnut	9	5 0	85	12·5	48
Red Pine	12	0	62	20	18
Walnut	9	4 0	45	12	15
White Pine	8	5 0	32	11	25
Elm	9	0	48	13	35
Yew	8	4 0	46	9	10
Beech	7·5	0	38	10·5	30
Plane	6	5 0	56	10	11
Virginian Cedar ...	6	5 0	51	7	9
Holm-Oak (green)...	5	60 0	90	7	17
Apple	3	4 0	86	3	10
Ordinary Oak	3	5 0	32	3	9

* Reduced Table. (Trans.)

It would have been difficult, after the first experiments recorded in the first four columns of the preceding table, to decide *à priori* if the considerable difference observed between the conductivities of different kinds of wood arise uniquely from a more or less hygrometric quality, or simply from a certain quantity of humidity concentrated in the interior of the wood which escaped the action of the stove, and which announces its presence after a certain time. This much is certain, that after submitting the samples of wood in question again to drying for two hours in the stove I could not obtain a galvanometric deflection after two hours'

sojourn in the moist cabinet, and it is only after fifteen hours' stay there that I obtained the results given in the fifth column of the table, which are the ratios of true absorption.

No. 3.
ON THE ACTION OF TWO CURRENT ELEMENTS.
By M. T. Bertrand.

Two parallel currents attract each other when their direction is the same, and mutually repel when their directions are opposed. Having enunciated this law, Ampère has immediately generalised its extension to two current elements, to which he may apply, whatever may be their relative direction, the idea of motion in the same direction or in contrary directions. Two currents are said to be in the same direction when they are propagated to the foot of a common perpendicular, or when they mutually approach; in contrary cases, they are in different directions. In adopting these terms, it is not exact to say that two elements in the same direction attract each other. As this assertion has been reproduced in all physical treatises, and as it serves as the basis of several important explanations, I believe it will be interesting (1) To show that it is in disaccord with Ampère's law; and (2) To solve the following problem:—

A current element being given, to find a point M in space in the direction that it is necessary to assign to another element in order that their mutual action may be attractive, repulsive, or nil.

Let us suppose the element ds placed at the origin of co-ordinates and directed along the axis X; let us seek the condition that an element of which the co-ordinates are x', y', z' are without action on ds. Designating by θ and θ' the angles formed by the two elements with the right line that joins them, and by ϵ the angle they form between them, the condition, according to Ampère's law, is—

$$(1) \qquad \cos \epsilon = \frac{3}{2} \cos \theta \cos \theta';$$

but, designating by r the radius vector and ds' the attracting element, we have

$$\cos \theta = \frac{x'}{r}$$

$$\cos \theta' = \frac{dr}{ds'}$$

$$\cos \epsilon = \frac{dx'}{ds'}$$

The equation (1) becomes

$$(2) \qquad 3\frac{x'}{r}\frac{dr}{ds'} = 2\frac{dx'}{ds'}$$

of which the integral is

$$(3) \qquad r^3 = A\,x^2,$$

the equation to a surface of revolution of which the axis is the axis of X, and of which the curve has for its equation, as related to polar co-ordinates,

$$(4) \qquad r = A\cos^2\theta.$$

Whatever may be the form and direction of a current wound on such a surface, the action upon an element placed at the vertex and directed along the axis will be nil. The presence of the arbitrary constant in equation (4) admits the relation of the surface with a known point in space, and there exists in each point an infinity of directions in which we may place an element ds' in such manner as to annul its action on the given element ds; these directions are all in the plane tangent to the surface found. It is clear that the action is maximum when the element is normal to the surface, and that it is for an element of given length and intensity, proportional to the cosine of the angle formed with the normal. If the element ds, placed along the axis of the surface, is directed exteriorly, any element starting from a point of the same surface and directed exteriorly will exert a repellant, and any element directed interiorly will exert an attractive, action.

PAGET HIGGS, LL.D. D.Sc.

GEOMETRICAL ILLUSTRATIONS OF OHM'S LAW.

By Prof. G. C. FOSTER, F.R.S.

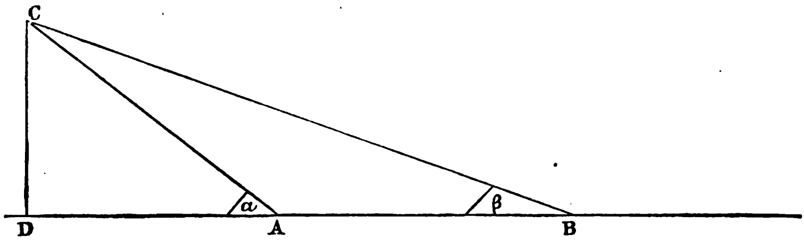
To find the *internal resistance* and the *electromotive force* of a battery without the use of tables.

I. By means of a *tangent galvanometer.*

A circuit is made up of a battery, tangent galvanometer, and set resistance coils. On making contact the galvanometer needle is deflected through an angle α; then a resistance R is added, and the deflection

falls to the value β. Take a point A in a straight line, and make angle $DAC = \alpha$. Make $AB = R$, and angle $DBC = \beta$. Let fall $CD + DB$.

Fig. 1.



Then D A measures original resistance of the circuit, and C D the electromotive force, because—

$$\frac{CD}{DA} = \tan\alpha = \text{1st current},$$

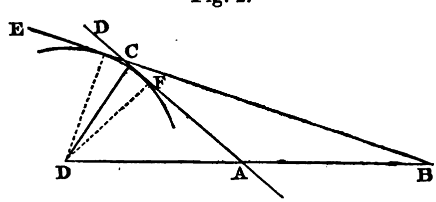
and—

$$\frac{CD}{DB} = \tan\beta = \text{2nd current}.$$

II. By means of a *sine galvanometer.*

Make $DAC = \alpha$, and $DBC = \beta$. Produce B C to E, and draw E D bisecting E C A. With centre D draw a circle to touch A C and

Fig. 2.



C E. The radius of this circle measures the electromotive force, and A D, as before, measures the internal resistance. For—

$$DF = \text{radius of circle} = DF',$$

$$\frac{DF}{DA} = \sin\alpha \text{ and } \frac{DF'}{DB} = \sin\beta.$$

Telegraphic Journal.

ARGENTINE TELEGRAPHS.

The following statistics of the telegraph system of the Argentine Republic are extracted from an official report by Mr. Charles Burton

(Hon. Secretary of the Society of Telegraph Engineers), Director-General of Telegraphs :—

Miles of Line constructed in each Year.

Under Contract.	1870.	7871.	1872.	1873.	1874.	Total Miles.
No. 1 .	129	362	397	124	—	1,012
„ 2 .	—	386	257*	—	53	696
„ 3 .	—	250	—	—	—	250
„ 4 .	—	186	—	—	—	186
„ 5 .	—	—	236	178	60	474
	129	1,184	890	302	113	2,618

Iron posts (Siemens's patent) have been placed on 1,448 miles of line, and for a distance of 1,170 miles wooden poles of this country, and in their natural state, have been used.

The total extension of the line amounts to 2,618 miles, with 5,218 miles of wires.

There are 58 offices open to the public, there being also instruments for service and official use in the Ministry of the Interior, in the Direction General, and in the Telegraphic School. There have also been offices in the International Exhibition in Córdoba and in Rio de las Piedras, the latter one having been removed to Rosario de la Frontera in consequence of the intermittent fevers, which prevented the clerks attending to their duties.

The preceding figures refer to the national telegraphs only, besides which the following have been constructed :—

	Miles.	
	Line.	Wire.
The Transandine, subventioned by the National Government .	617	1,234
The Provincial (Telégrafo del Estado)	250	500
The Central Argentine Railway Telegraph	248	406
The Western do. do.	151	259
The Great Southern do. do.	150	218
The Northern do. do.	18	36
The Ensenada do. do.	32	64
The East Argentine do. do.	30	60
The River Plate Telegraph Company	32	64
Total	1,528	2,841

By adding the extension of the above to the national telegraphs we have a total for the Argentine Republic of 4,146 miles of line and 8,059 miles of wire.

Transmission Signals.

As the instruments in our service are all "Morse," the alphabetical signs in use are simply an adoption of those used in Europe by the administrations which have subscribed to the International Convention.

Intentional Damages.

The lines suffer frequently from mischievous injuries, without it being possible to discover the delinquents, but they must be committed by—1st. Those who do not fully understand the serious results of their acts; 2nd. Linemen dismissed for bad conduct; 3rd. Ignorant people, who think that by intercepting the national lines they benefit some other undertaking; and, 4th. Persons occupied in subversive politics, who cut the telegraph whenever they expect the transmission of any governmental order which might impede the realisation of their projects.

Once a wire appeared cut on the line which runs to the northern provinces, in the section between Rosario and Córdoba, and alongside the Central Argentine Railway. The linemen revised several times without noting any defect, either from the train or on horseback: it was necessary to order them revise more minutely on foot, when they found that a piece of wire had been cut out, and replaced by a curtain-cord of the same size, and painted to resemble the line-wire.

At other times, in the same section and in that from Buenos Ayres to Rosario, as also in other parts, there have been found fine guitar-strings placed with much precision uniting the wires to the iron post, and in such points as were with difficulty seen from the road.

One interruption which was produced between San Nicolas and Rosario merits special notice. A guitar-string of metal, which had been used to place the two line-wires in contact, had been wrapped first in a piece of raw hide, in such a manner that the linemen noticed only the strip of hide, but knowing that this alone could not be the cause of the interruption; thus causing a considerable delay in the discovery of the obstacle.

The provincial authorities, which in all parts do not desire to acknowledge the independence of Federal Government *employés*, sometimes interfere with the working of the telegraphs with exaggerated pretensions, which perhaps on some occasions might be considered violence. But, I

am glad to say, these difficulties have always disappeared the moment the Federal Government took the question in hand.

Mechanical Department.

The workshop for the repairs of instruments, &c. has continued to be attended to by the Sub-Director until a few days ago, when a mechanician was appointed who had been occupied for fourteen years in a similar employment.

Our Morse instruments have been constructed with the utmost care. For this reason not one has been rendered unfit for work; only such damages have been caused as are due to the inexperienced hands of the learners, who sometimes have to manipulate them, but the injury caused has been repaired immediately.

New Lines.

The construction of our land-lines may be said to be concluded for the present. All the provinces of the Republic are placed in communication with one another. So many lines having been constructed simultaneously and in equal conditions, the result is, that the trunk of this telegraphic tree being only of equal dimensions with its numerous branches, it cannot conveniently despatch the communications with which these overcharge it. Thus we have the telegraph of two wires uniting the cities of Buenos Ayres and Rosario, which might serve to maintain satisfactorily the communication between these two points, obstructed with the torrent of despatches proceeding from the lines in all the other provinces furnished with telegraphs possessing an equal nnmber of wires.

THE ELECTROMOTOGRAPH.

A NEW DISCOVERY IN TELEGRAPHY.

To the Editor of *The Scientific American* :—

In my new system of telegraphy it would seem that power was obtained, or that electricity had been passed into a new mode of motion, as with magnetism, but this is only apparent, not real, if I understand it right.

The electricity, acting by electrolysis, changes the nature of the surface of the paper, either by depriving it of some constituent, or the hydrogen, in conjunction with the metal and paper, forms substitution

compounds, the surfaces of which are smoother than the paper in its natural state, in the manner that the surface of rough paper is made smooth by dipping it into sulphuric acid. The strangest thing connected with this phenomenon, however, is this:

In trying to ascertain what caused the lever to move, whether it was by reducing the lead by hydrogen to a finely-divided powder that acted as a lubricant, or whether the nature of the surface of the lead were changed by the absorption of hydrogen, like palladium, or whether the effect were due to the effort of the gases to escape from under the lever,—I was led away from these notions by finding that platinum, with sulphate of quinine, will likewise show the movement. It then struck me that the nature of the paper was changed by the electrolysis. To test this, I had a long message received over the Automatic Telegraph wire from Washington (this wire runs into my laboratory at Newark), and recording the same on ordinary chemically prepared paper. The speed with which the message was sent from Washington was about 800 words per minute, and the colourations forming the dots and dashes were rather faint. I then passed the strip into the electromotograph (I use this name for the want of a better one), the colourations being in a direct line with the lead point. On rotation of the drum, and when no colouration was under the lead point, the lever was carried forward by the normal friction of the paper. But the moment a colouration passed under it the lead point slid upon the paper as upon ice, the friction was greatly reduced, and the lever moved in an opposite direction to the rotating drum.

In this experiment no battery was connected to the instrument. This proves that electrolysis produces a change in the nature of the paper.

I afterwards found that, if a tin pen were used to receive the message from Washington, although no marks were seen, the paper appearing unchanged, yet, on passing the paper through the instrument, the movement of the lever was more marked than before. Receiving the message with a lead pen did not give so good results, although lead is the best when used direct, standing at the head of the twelve metals tried. The next is thallium. On paper moistened with aqueous solution of pyrogallic acid, tin is equal to thallium. Of all the solutions yet tested, potassic hydrate has been found to give the most marked results. The second best is sulphate of quinine. Third, hydrochlorale of rosaniline oxidized and discoloured by nitrous acid.

A peculiarity of the quinine solution is that platinum shows an action, and shows it when either oxygen or hydrogen is evolved on its surface.

With hydrogen the friction is lessened, as with all other metals; but with oxygen the friction is increased. This is so with all the metals subject to oxidation; but it appeared strange at first that it would show with a metal upon which the nascent gases had no effect.

With a lead point and a solution of the disinfectant known as bromo-chloralum, the evolution of oxygen increases the friction of the paper enormously.

Silver seldom shows a movement with any solution; and when it does it is very weak.

Sulphuric acid shows least movement with any metal.

It appears to be a matter of indifference as to the character of the metal used for the drum, which acts as one of the decomposing electrodes. Considering that the lever will close a secondary circuit under the great pressure used upon the lever, its sensitiveness to electricity is wonderful. With a delicately constructed machine moved by clockwork, which I have nearly finished, I have succeeded in obtaining a movement of the lever sufficient to close the local dircuit with a current (through over one million ohms, equal to 100,000 miles of telegraph wire), which was insufficient to discolour paper moistened with potassic iodide or move an ordinary galvanometer needle. Messages may be read from the sound of the lever when the most delicate telegraph magnet shows no current.

The uses of this instrument are many; in fact, it gives an entire new system of telegraphy.

As no secondary currents are generated, as with an electromagnet, to prevent the instant magnetization or demagnetization of the iron cores, and electrolysis being instantaneous, it is obvious that the lever will respond to signals transmitted with great rapidity. I have succeeded in transferring signals from one circuit to another at the rate of 650 words per minute; hence it may be used to repeat the rapid signals of the automatic telegraph into secondary circuits.

By attaching an ink wheel to the extremity of the lever, opposite a continuous strip of paper moved by clockwork, messages transmitted at a speed of several hundred words per minute may be recorded in ink. By attaching a local circuit to the repeating points, and adding thereto a sounder, it may be used as a Morse relay to work long lines of telegraph. T. A. Edison.

Newark, N. J., August, 1874.

ON THE FALL IN PITCH OF STRAINED WIRES THROUGH WHICH A GALVANIC CURRENT IS PASSING.

By Dr. W. H. Stone.*

The object of this paper was to apply the vibrations of sound to the measurement of electrical currents, and to distinguish what was due to heating effects from those caused by alteration of elasticity.

Strings of brass and steel, such as are used for pianofortes (No. 16 gauge), were stretched, by means of wrest-pins, across a resonant box, over bridges surmounted by brass bearings, and tuned to unison. On passing a current from two or more Grove's batteries through them, a very marked fall in pitch was obtained. The vibrating string being 24 inches long, and tuned to two-foot C, the tone sank above a fourth in steel and a major third in brass.

This result being a compound of actual lengthening by heat and of other causes, it was, in a second experiment, endeavoured to eliminate the former element by straining similar strings between the same bridges by means of a weight. This was attached to the arm of a bent lever, to the short end of which the string was made fast. By shifting the position of a four-pound weight along the arm, very accurate unison, or definite periodicity of beats, could be obtained. When the current from the battery was passed through this string, free to expand by the falling of the weight, and therefore at a constant tension, a fall of pitch was still noticed. There was also a very marked loss of tone, which, on approaching a red heat, amounted to a total extinction of sound.

A third experiment exhibited the changes of electrical resistance in a wire subjected to variations of strain. The wire was accurately balanced against another resistance in a Wheatstone's bridge, and the spot of light from a mirror-galvanometer joining the two circuits thrown on the screen. On suddenly increasing the tension and raising the musical pitch of the string, the galvanometer was visibly deflected This was not an effect of heat (since the balance had been brought about during

* Read before the Physical Society, May 9, 1874. Communicated by the Society.

the passage of the current), and must be due to altered molecular state caused by the strain.

It was incidentally noticed that, when beats were produced by two strings on the same sonometer, they continued to be sensible to the touch by laying the hand on the instrument long after, from diminution of amplitude in the vibration, or from slowness in the beats themselves, they had ceased to be audible. This afforded a good demonstration of the continuity of sensation in touch and hearing.—*Philosophical Magazine.*

ON CERTAIN REMARKABLE MOLECULAR CHANGES OCCURRING IN IRON WIRE AT A LOW RED HEAT.

By W. F. Barrett, F.C.S.,

(Professor of Experimental Physics in the Royal College of Science, Dublin).

In the "Proceedings of the Royal Society" for January 28, 1869, Mr. Gore published the important fact, that, when an iron wire is heated to bright incandescence and then allowed to cool, *a momentary elongation*, or, as Mr. Gore believed, diminution of cohesion, of the wire occurs just after it has begun to contract by cooling. The main points in Mr. Gore's paper are as follows: A thin iron wire fixed at one end to a binding-screw is attached at the other to an index which multiplies any motion of the wire; the wire is strained horizontally by a feeble spring; and matters are so arranged that the wire can be heated by an electric current or by a row of gas jets. When heated, the wire expands and the spring pulls the index over. A sketch of the instrument is given in the Philosophical Magazine for July 1869. Mr. Gore states that *no* anomalous action is observed on *heating* the wire to bright incandescence; but, when the heating is discontinued and cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk or kick, indicating a momentary elongation of the wire during the progress of its contraction. This effect is perfectly certain, and always occurs at this particular temperature. Mr. Gore states that iron wire of a certain thinness and a certain tension of the spring is necessary, and

that the phenomenon is apparently *confined to cooling iron*, no such change being evident during the heating or cooling of wires drawn from the wide range of other metals he has examined. Further, Mr. Gore has investigated the production of induced currents during the cooling of magnetized iron bars, one portion of which had been heated to redness; and the result showed that the iron bar "suddenly increased in magnetic capacity during cooling at a particular temperature of moderate red heat."

Having occasion to show Mr. Gore's discovery in the course of a lecture delivered some eighteen months ago to the Dublin Royal Society, Mr. Gore kindly furnished me with his own apparatus. By attaching to the movable cross-piece a light mirror, from which a brilliant ray of light was reflected to a scale on a distant wall, the effect sought was not only vastly magnified, but one or two new facts also revealed themselves. (i.) During the heating of the wire a slight and momentary retrogression of the beam was noticed at the temperature corresponding to the powerful jerk that occurred on cooling: some smaller tremblings of the beam were noticed at higher and lower temperatures; but these seemed due to irregular heating and cooling. (ii.) It was evident that the anomalous deportment of the iron occurred approximately at the critical temperature when iron undergoes its principal magnetic change.

Mr. Gore having stated in a letter to me, written in May 1872, that he had no intention at present of making any more experiments in the direction of his discovery, and adding, that the subject was quite open to me, I felt at liberty to pursue the inquiry thus suggested. It was not, however, till this autumn that I could find the necessary leisure; and the following results were then obtained.

My best thanks are here due to Professor Guthrie for his hearty welcome to use his laboratory at South Kensington, where the experiments have been conducted.

I.

Employing twenty Grove cells, I have had no difficulty in obtaining this anomalous behaviour with *moderately thick* iron wires. These have the advantage of allowing the effect to be studied more leisurely, the phenomenon sought for occurring several seconds after the interruption of the current. The temperature at which the momentary jerk occurs seems to be lower in thick wires than in thin ones, the critical point being

a moderately bright or cherry-red heat in thin iron wire, say No. 23, and a very dull red heat in thick wire, say No. 20; the latter wire is, in fact, in the stage just preceding obscurity when the effect occurs. The internal temperature of the thicker wires is no doubt masked by the cooling of their surface, whereas in thin wires the cooling throughout is extremely rapid, and moreover the transitions of temperature cannot be so well noted.

II.

With No. 21 hard iron wire I have had no difficulty in obtaining the jerk during heating. In this case the movement is in the reverse direction of that which occurs during cooling; that is to say, it indicates a momentary *retraction*, occurring, as closely as can be judged, at the same temperature at which the *elongation* takes place in cooling.

With a steel wire 25 centems. long and No. 22 B wire gauge, the following observations were made—a battery of ten Grove cells being used, capable of raising this length of wire to a bright white heat. The index stood at 0 on the scale before contact was made, the wire being cold. After contact, as the wire became heated, the index regularly passed to 24; here it promptly retreated to 22, then steadily passed onwards to 34, the wire now glowing white-hot. Breaking contact, the index returned regularly to 20, then rose suddenly to 27, after which it continued its backward course till it finally rested at 2, the wire now being cold again. The action of the spring stretches the wire when hot, hence the index does not return to zero. Allowing for this stretching, the figures would be proportionally lower where the jerk occurs on cooling, viz., 18 to 25.

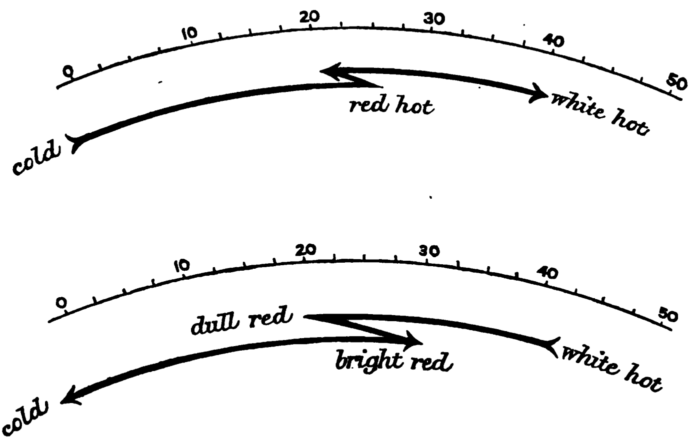
Here are two more out of many experiments with the same wire:—

1. Wire cold; contact made; index rose from 0 to 25, jerked back to 23, then rose to 33; wire bright red.
   Wire bright red; contact broken; index fell from 33 to 19, jerked forward to 25, then fell to 4; wire cold.

2. Wire cold; contact made; index rose from 0 to 25, jerked back to 23½, then rose to 32; wire bright red.
   Wire bright red; contact broken; index fell from 32 to 20, jerked forward to 24½, then fell to 4; wire cold.

The following diagram (fig. 1) illustrates the motion of the index or heating and cooling the wire.

Fig. 1.



Releasing the tension of the spring, the forward motion on cooling is, as might be expected, much lessened, whilst the jerk back is scarcely affected. Increasing the tension of the spring, the forward jerk is correspondingly increased, and the backward jerk diminishes, and can be made to disappear.

III.

Is this anomalous action, then, due to a momentary change in the cohesion of the wire? If so, at a certain point during the progress of *heating*, the molecules of iron have a sudden *accession* in elasticity, and at an approximately corresponding point during *cooling* they incur a sudden *loss* in elasticity. If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling.* Fixing one end of the wire and bending the other

* A striking lecture experiment may be made by simply stretching some harpsichord wire between two supports, and heating the wire to whiteness by a current. On allowing the wire to cool it gradually straightens itself till just as it reaches the point of obscurity, when it suddenly drops for an instant. It is extraordinary that this action has not been frequently observed.

extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen; the sudden outward thrust on cooling is very conspicuous. Heating the wire by gas-flames, the same result is given.

All kinds of iron do not exhibit this behaviour; and some show it in a more or less marked degree. I have not been able to detect any change, in heating or cooling, in certain specimens of good soft iron wire; but in hard iron wire, and notably in steel wire, it is very apparent. The wire, moreover, requires to be raised to a very high temperature before the jerk is seen on cooling. I have not observed the momentary elongation on cooling when the wire has only been heated to a point *just beyond* that at which it would otherwise occur. The behaviour of iron wires of different degrees of purity and of widely-different thicknesses are points I hope to examine in a subsequent inquiry. I may here also mention that the precise magnetic condition of the iron at the moment at which the jerk occurs, together with its electric resistance and its thermo-electric position,* are questions upon which I have already made some experiments, but not enough to justify the publication of any results at present.

IV.

On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that, just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.* Necessarily no change of this kind can be observed on heating; but the reglowing of the wire on cooling is most uniform and conspicuous.† The wire must first be heated to whiteness; and then, being allowed to cool, just as it reaches a point of barely visible redness, a sudden cherry-red glow takes place, passing as a wave of heat from one end of the wire to the other, or from both ends to the centre. The measured progress of this wave of temperature along the wire is extremely beautiful to observe. On first sending the current through the wire, the heating

* Professor Tait's remarkable investigation on this point is alluded to subsequently.

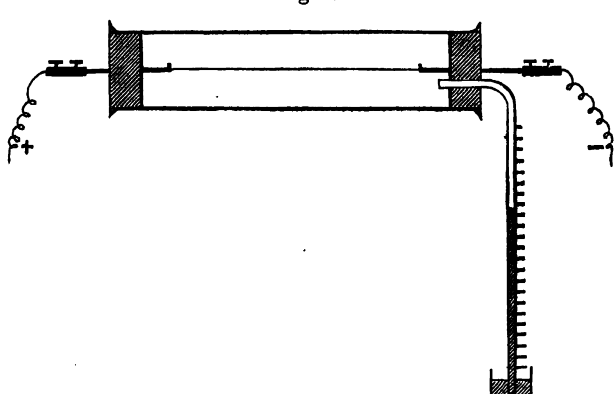
† Nevertheless during heating I thought I detected a momentary pause in the progress of the reddening of the wire, just after incandescence had been reached.

begins at one extremity and runs along to the other; on breaking contact, this reheating sweeps along the wire in the contrary direction. This peculiar movement, therefore, may be caused by the unequal thickness of the wire, though I do not think this is the explanation, as the reheating would then move in the same direction as the heating (namely, from the thinner to the thicker parts of the wire), and this is not the case. I hope shortly to investigate this further.

When the wire is heated by a row of gas-flames, the same results take place, although the heating by the battery is a far neater and more satisfactory way.

It is a real accession of temperature, a sudden increase in thermal as well as luminous radiation. This is evident from the following experiment: A wide glass tube (fig. 2) was fitted with corks at each end, so that the iron wire could be inclosed air-tight within the tube. At one end the cork was perforated to allow the insertion of a narrow glass tube bent at right angles, the lower end of which dipped into coloured water. On heating the wire to whiteness by the current, some of the inclosed air was expelled, and on breaking contact the liquid rushed up the tube, but midway suddenly stopped in its course, and was depressed some two inches. At this moment the assistant, who was watching the wire, gave notice the wire drooped and glowed again. There is no difficulty in repeating this experiment, nor in exhibiting its action to a large class. Wherever the momentary expansion of the wire is feeble or absent, there

Fig. 2.



likewise this *recalescence*, as it might perhaps be termed, is also feeble or absent. I am anxious to procure wires of nickel and cobalt; for, from

the reasons set forth in another paper, the action might certainly be expected to occur in those metals as well as in iron, probably with nickel at a lower and with cobalt at a higher temperature than in the case of iron.

V.

Besides the molecular changes here detailed, there are indications of the existence of other disturbances during the heating and cooling of an iron wire, notably *the emission of a peculiar dry crackling sound*, like the crepitation that occurs on magnetizing and demagnetizing iron; and this, too, occurs at the critical temperature. But so many matters of interest have arisen during this investigation, that the present paper can only be regarded as the results of a preliminary inquiry.

VI.

The molecular disturbances to which iron is thus seen to be subject at a particular temperature are no doubt associated with an even wider range of phenomena than are here indicated. Professor Tait's experiments, which were read by me after most of the foregoing facts had been obtained, show that iron exhibits a most remarkable and anomalous *thermo-electric* deportment at a red heat. In his Rede Lecture* that eminent physicist points out that "the cause of this is, that while, as Sir W. Thomson discovered, the specific heat of electricity in iron is negative at ordinary temperatures, it becomes *positive* at some temperature near low red heat, and remains positive till near the melting-point of iron, where it appears possible from some of my experiments that it may again change sign." And further on Professor Tait suggests the idea "that iron becomes, as it were, a different metal on being raised above a certain temperature. This may possibly have some connexion with the ferricum and ferrosum of the chemist, with the change of magnetic properties of iron, and of its electric resistance at high temperatures." Professor Tait adds the interesting fact that he has found an anomalous thermo-electric behaviour in *nickel* similar to that in iron, and, as one might venture to anticipate, at a much lower temperature.

Thus two separate lines of inquiry have converged on the same point—namely, that a profound molecular disturbance takes place in iron at a low red heat. In connexion with future theories of magnetism, this fact is likely to be of considerable importance, inasmuch as it seems probable

* Nature, June 12, 1873.

that this disturbance is confined to the magnetic metals, and that it occurs at or about the temperature when they leave or re-enter this condition.—*Philosophical Magazine.*

ON THE RELATIONSHIP OF THE MAGNETIC METALS.

By W. F. Barrett, F.C.S.*

The remarkable similarity in the chemical and physical properties of the magnetic metals has no doubt often attracted attention; but I am not aware that any definite collation of these properties has ever been made. This I propose briefly to do in the following paper. The extraordinary homology these metals are thus seen to exhibit furnishes instructive evidence concerning the molecular state of a magnet.

By magnetic metals I mean those metals which possess magnetic properties under ordinary circumstances—namely, iron, nickel, and cobalt.

First we will compare their physical characteristics. The *specific gravity* of the thirty-eight known metals ranges from lithium 0·59 to platinum 21·5, a difference of nearly 21; whereas the specific gravity of iron is 7·8, nickel 8·3, and cobalt 8·5, an extreme difference of only 0·7. The *specific heat* of these three metals is also nearly identical; and their *atomic heat* is the same. Their *conductivity for sound* is almost absolutely the same; and, so far as their heat and electric conductivity have been determined, they are also alike. Their *dilatation by heat* is the same, and so also is the amount they lengthen by mechanical strain. They belong, I believe, to the same system of *crystallization*, namely the monometric, though too little is known on this point. The enormous *cohesive power* of iron, nickel, and cobalt in the solid state signalizes these substances as the most *tenacious* of metals. To overcome this cohesion a very high and somewhat similar temperature is required, and their *melting-point* is only exceeded by the platinum group of metals. Their refractory character renders them not volatile even at the temperature of the hottest furnace. When, however, they are volatilized by means of the electric spark, their incandescent vapours yields a *spectrum* which has a close and curious resemblance. This teaches us that the molecules of these bodies, freed from the thrall of cohesion, vibrate in periods which are closely akin.

* Communicated by the Author, having been read before the British Association at Bradford, September 1873.

A comparison of the *chemical* properties of the same metals furnishes a similar result. The ratio of the combining weight of the metallic elements ranges from lithium 7, to bismuth as 210, or a difference of 203. When we compare the magnetic metals, we find the combining weight of iron is 56·0, nickel 58·5, and cobalt 58·5, or a difference of only 2·5. Chemists class these three metals in the same group, from the similarity of their chemical behaviour, and also the identity of their combining energy or atomicity.

In strong nitric acid iron becomes endowed with a so-called passive condition, not acted upon, as it is in the dilute acid. Likewise I find nickel is capable of assuming a passive state in strong nitric acid. Cobalt, it is true, was violently acted upon under similar circumstances; but that, I believe, was due to the fact that the cobalt contained iron largely, and so an electrolytic action was probably set up. I have been unable to obtain pure cobalt—a very difficult matter, I believe.

A series of very similar chemical compounds are formed by these metals, mostly characterised by the brilliancy of their colour. The protosalts of iron are generally bluish green, of nickel emerald green, and of cobalt of a rose-colour. It is, moreover, a well-known fact that this rose-colour of certain cobalt salts passes into a ***bright green*** when they are warmed. Now, when the metal cobalt is moderately heated, it ***increases*** in magnetic power, thus differing from its congeners iron and nickel, which are in their maximum magnetic condition at the ordinary temperature, and at ordinary temperatures present the green-coloured salts.*

What has been said concerning the likeness of iron, nickel, and cobalt, in many respects holds true of ***manganese*** and ***chromium***, also feebly magnetic metals. Placed in the same group with the former metals chemically, they are physically characterised by their extraordinary tenacity and difficult fusibility. Manganese has lately been used to replace nickel in the alloy of German silver, and with excellent results I am informed. It is also worthy of note that the compounds of these five metals are conspicuous by the brilliancy of their colours, all their salts exerting a selective absorption on light, and their oxides dissolved in borax yielding well-known and characteristic tints—a comparatively rare feature outside this group.

Further, it is well known that the ores of cobalt and nickel are almost

* The therapeutic effect of the salts of these metals one would expect to be somewhat similar; and in confirmation of this I hear that nickel has lately often been used advantageously to replace the medicinal properties of iron.

invariably found associated in the earth and with difficulty separated. It is also noteworthy that both nickel and cobalt are usually present in meteoric iron—the average composition of meteorites being 90 per cent. of iron, 8 per cent. of nickel, and 0·5 per cent. of cobalt, curiously enough often with a trace of the other feebly-magnetic metals, manganese and chromium.

This uniform coincidence in the properties of iron, nickel, and cobalt, suggests the practical inference that nickel and cobalt might be obtained in a malleable and ductile condition when submitted to a process similar to that by which wrought-iron is produced. At present it is impossible to procure nickel or cobalt wire, though there seems no reason why they could not be made if a demand arose. Nickel wire would probably prove very useful from its high tenacity and comparative freedom from oxidation.

The following table sums up some of the most striking points of contact in the physical properties of the three magnetic metals *par excellence*:—

Table showing the physical relationship of the magnetic metals.

Substance.	Density. Water = 1.	Atomic weight. H=1,	Specific heat. Water = 1.	Atomic heat.	Dilatation by heat*.	Dilatation by strain*.	Conductivity for heat. Silver=1.	Conductivity for sound*. Air=1.	Tenacity and melting-point.
Iron...	7·8	56·0	0·1138	6·38	·0926	·0387	·168	15·3	Very high.
Nickel	8·3	58·5	0·1091	6·33	·0899	·0394	·131	14·9	"
Cobalt	8·5	58·5	0·1070	6·26	·0981	·0436	·172	14·2	"

From this table it is evident that the molecular constitution of the magnetic metals is essentially alike, largely differing from bodies which are not magnetic. And, this being so, further evidence is afforded that the evolution of ordinary magnetic phenomena is in some way associated with the peculiar and similar structure of the molecules of iron, nickel, and cobalt.—*Philosophical Magazine.*

* For the figures in this column I am indebted to a paper by M. A. Masson, in the *Annales de Chimie et de Physique* for 1858. In the heat column the decimal would, of course, have to be moved four places to the left to express the coefficient for 1° C. The dilatation by strain was of one metre of the body under a weight equal to itself.

ON EARTH-CURRENTS.

By L. Schwendler.

Mr. Schwendler said that the phenomenon of earth-currents seemed to be intimately connected with the earth-magnetism and its variations.

He would, however, point out from the beginning, that, though the two phenomena, "earth-magnetism" and "earth-currents," were undoubtedly connected with each other, it was by no means established as yet that they were cause and effect, or, what certainly seemed to be far more probable in the present state of knowledge on the subject, parallel effects of *one* and the *same* general but entirely unknown cause.

The three elements of the earth-magnetism, intensity, inclination, and declination, had been quantitatively and most accurately determined in almost all civilized parts of the world (Calcutta excepted) by the introduction of Gauss' and Weber's well-known system of magnetic measurements; and though the results obtained had been very general and satisfactory, establishing the most interesting facts of diurnal and secular periods of variation in the three magnetic elements, and had also been of direct practical benefit to navigation, still the physical nature of the phenomena had not been unveiled by these observations. To solve the problem it would seem that quantitative measurements of other phenomena, directly or indirectly connected with it, were required, and it was most fortunate that at least one such phenomenon not only existed but was even susceptible of accurate measurement: he meant the "earth-currents."

The chances of giving a true physical explanation of any phenomenon, he observed, increased in geometrical progression with the number of phenomena directly or indirectly connected with the one to be explained, supposing that they were all susceptible of accurate measurement.

In this particular case he had to deal with two such parallel phenomena, the magnetism of the earth, quantitively ascertained for more than forty years past, and "earth-currents," sadly neglected.

He said he was perfectly aware why "earth-currents" had not been measured, and then, after mentioning the special purpose of his paper, *i.e.* not to start a fresh theory of the earth-magnetism with the scanty and imperfect material available, but to lay before the Society some more facts connected with its parallel phenomenon, the earth-currents in the telegraph lines, which had been quantitatively measured during the last

six years in widely different parts of the empire, Ceylon included, he proceeded as follows :

"That it was well known that from time to time telegraph lines, overland, underground, and submarine, were affected by what had been called 'magnetic storms,' *i.e.* by very strong currents passing through the wires and overpowering entirely those used for signaling, with which electrical disturbances co-existed magnetic variations far exceeding the limits generally observed when no such electrical disturbances exist, and very often accompanied in the northern (and most likely also in the southern) part of the planet by vivid auroras. Now these currents observed in the telegraph lines were 'earth-currents.'

"For instance, on the 10th November, 1871, and on the 4th February, 1872, earth currents of considerable strength had been observed in all the lines throughout India, and the submarine cables terminating on its shores. These great electrical disturbances were by no means local, but existed almost simultaneously throughout the earth, showing us a most interesting feature of our planet.

"The fact of the secular changes of the earth magnetism occupying such a long period as about 1,000 years (the principal magnetic pole moving round the astronomical pole in 1,000 years) pointed most probably to a cause external to the planet. If he were allowed to follow his own imagination, he would say that earth magnetism, its diurnal and secular variations, auroræ boreales and australes and electrical disturbances, weak or intense, in the planet, were all due to the movement of the earth and of the heavenly bodies generally. That the great electric convulsions observed from time to time were nothing but the telegraph signals transmitted from far distant regions to our planet, indicating great physical changes in the universe long before, if ever, they could be felt by the more rough instruments—light, heat, and gravitation—at present the only means by which we recognise our kinship with the outer world.

"It could be, therefore, easily perceived how important it was to investigate such a phenomenon (probably of all the most widely connected) by direct measurements.

"Now if such electrical disturbances only existed by fits and starts, as was the case during magnetic storms, it would be almost hopeless to attempt a general system of measurement. This was, however, fortunately not the case, since these earth-currents, which during magnetic storms became so violent, seemed to exist permanently, only of very

feeble strength, and it was on this subject that he would give some observed facts."

The general outline of the rest of Mr. Schwendler's communication will be best given in extracts from his paper, which will be printed in full in Part II. of the Journal.

Mr. Schwendler says:

"The currents observed at all hours of the day and all seasons of the year, in every line throughout India, may be obviously due to many different causes acting separately or conjointly. These currents I have designated 'natural currents,' to indicate the fact of their being in the lines without any direct, or at least intentional, human agency. The causes which may produce natural currents in telegraph lines are—

"1. Galvanic action between the earth-plates.
"2. Polarization of the earth-plates by the signalling currents.
"3. Polarization of badly-insulated points in the line.
"4. Atmospheric electricity.
"5. Thermo-electricity.
"6. Inductive capacity.
"7. Voltaic induction.
"8. Earth currents.

"The latter must be considered as produced by an actual difference of potentials between the two points of our planet with which the ends of a telegraph line are in contact.

"Surely if these 'earth-currents' do permanently exist, and further, if they are strong enough to overpower the others, which are evidently of a much more accidental and less permanent nature, then a large number of quantitative observations, judiciously reduced and conveniently compiled, should at least show the tendency of the general law that governs them in strength and direction, leading perhaps finally to the true explanation of the earth's magnetism and the causes of its variations.

"Such were in short my reasonings when in 1868 I was entrusted by Colonel Robinson, the Director-General of Telegraphs, with the introduction of a system of testing the lines in India, and, although the practical objects of that system had nothing whatsoever to do with the solution of the problem, yet the fact that in each test measurements had to be made with positive and negative currents (for the very purpose of eliminating the influence of the natural currents) secured all the data necessary for the quantitative determination of the electro-motive force

in the line, to which the natural current must be considered proportional, involving only a slight additional calculation without any extra observations. To this end the necessary provisions were made and instructions issued; and in this manner more than 10,000 electromotive forces, producing the natural currents in the lines of India, have been calculated from the tests made between 1868 and 1872, and are now at our disposal; and, although the results of these numerous observations have not as yet been all analysed, or even compiled, yet in many special cases, and for limited periods, this has been done, and from these we are justified in stating the following as facts:—

"1. All the lines in India are affected by natural currents.

"2. From more than 10,000 observations it has been established that the prevailing flow of these currents between any pair of stations is as of a copper current from the east to the west; but which is the true direction, or that of maximum intensity, and further, whether there is only one such direction, has not been computed as yet.

"3. The strength of the natural current in one and the same line is very variable.

"4. The direction of the natural current in one and the same line, though also variable to a certain extent, is, however, far more constant than its strength, and out of a number of observations there is generally a marked preponderance of currents flowing in the same direction.

"5. The variation in strength and direction of the natural currents in parallel lines of the same length, is far more uniform than might have been expected, considering the many accidental influences to which long overland lines are exposed.

"6. The prevailing direction of the natural current in any line is generally also the direction of the maximum current observed, but this is not the case invariably.

"These general facts point to *one* probable conclusion, namely, that '*earth-currents*' do permanently exist in the lines of India, though they are often, and under certain circumstances even much, obscured by many other causes, of commensurate magnitude, but more unstable and accidental in character.

"For example, the two Railway lines between Bombay and Madras, one of which is very perfect in insulation, while the other is quite the reverse, both exhibit a copper current flowing permanently from Madras towards Bombay; and this fact, having been ascertained from a large number of tests, extending over a considerable period, and made from both Marasd

and Bombay, proves that the cause is a general one with respect to time, and that the method and place of measurement do not influence the direction of the current observed. Further, as one of the wires is used for the through traffic towards Bombay, while the other is used for the through traffic towards Madras, and as both circuits are worked with copper currents, the natural currents, which flow in the same direction in the two wires, certainly cannot be due to the polarisation of the earth-plates or of faulty places in the lines. The average electromotive force in these wires is about 4·5 Daniells, and maxima of 15 and 20 Daniells are occasionally reached.

"I consider it, therefore, established that '*earth-currents*' do permanently exist in the lines of India, their general drift being from east to west, and that we should be now justified in establishing a special system for the purpose of observing them, according to a uniform plan and with improved test methods."

Mr. Schwendler concluded by saying, that, based on the facts above stated, he had proposed to the Council of the Asiatic Society to urge on Government the introduction of a system of measurement of *earth-currents*; that the Council had received the proposal most warmly, and had appointed Colonel Hyde, Mr. R. S. Brough, and himself, to work out a practical system; and that Colonel Robinson, the Director General of Telegraphs, had intimated his kind co-operation in the matter.—*Proceedings of the Asiatic Society of Bengal.*

THE SOCIETY OF TELEGRAPH ENGINEERS.

The Society of Telegraph Engineers is established for the general advancement of Electrical and Telegraphic Science, and more particularly for facilitating the exchange of information and ideas among its Members.

Any person desirous of being admitted into the Institution must be proposed and recommended according to a form, in which the name, usual residence, and qualifications of the Candidate (comprising a sketch, with dates, of his professional career, enumerating the works on which he has been engaged, and the positions he has occupied), must be distinctly specified. This form must be signed by at least four Members, certifying a personal knowledge of the Candidate.

The proposal of any foreigner, who may be desirous of joining the Institution, must be signed by at least one Member, certifying personal knowledge of the candidate and a full conviction of his qualifications.

The Society consists of Members, Foreign Members, Associates, Students, and Honorary Members.

Members.—Every Member shall come within one of the following conditions :—

(a.) He shall have been regularly educated as a Telegraph Engineer, according to the usual routine of pupilage, and have had subsequent employment for at least five years in responsible situations.

(b.) Or he shall have practised on his own account in the profession of a Telegraph Engineer for at least two years, and have acquired a degree of eminence in the same.

(c.) Or he shall be so intimately associated with the science of Electricity or the progress of Telegraphy that the Council consider his admission to Membership would conduce to the interests of the Society.

Associates shall be of more than twenty-one years of age, and this class shall include persons whose pursuits constitute branches of Electrical Engineering, who are not necessarily Telegraph Engineers by profession, but who are, by their connection with Science, qualified to concur with Telegraph Engineers in the advancement of professional knowledge.

Students shall be persons not under eighteen and not over twenty-one years of age who are serving pupilage to a Telegraph Engineer, or who are studying Natural Science, and are duly recommended by a Member.

Honorary Members shall be either distinguished individuals, who from their position are enabled to render assistance in the prosecution of Telegraphic enterprises, or persons eminent for science and experience in pursuits connected with the profession of Telegraphy, but who are not engaged in the practice of that profession.

Foreign Members are also admitted, paying a subscription of £1 a-year.

Every Member shall contribute the sum of Two Guineas, every Associate the sum of One Guinea, every Foreign Member £1, and every Student Half-a-Guinea, annually, to the Society.

Any Member or Associate absent from the United Kingdom nine months out of the year shall contribute the sum of £1 per annum.

Honorary Members shall not be required to pay any contributions.

The annual contributions shall be payable in advance on the 1st of January in each year.

Any Member may compound for his annual subscription by the payment in one sum of Twenty-one Pounds. Foreign Members and Associates may also compound by the payment of Ten Pounds. All such compositions shall be invested in the names of three trustees, and the interest alone shall be appropriated to the current expenditure of the Society, at the direction of the Council.

The Meetings of the Society are held, by the kind permission of the Council of the Institution of Civil Engineers, at their Institute, 25, Great George Street. The Session commences annually on the second Wednesday in November, and the Meetings are continued during the second and fourth Wednesdays in each month until May.

At these Meetings original communications are read, descriptive of Electrical and Telegraphic work, or of any branch connected with Electrical Science; afterwards, the merits of the communications are discussed. These papers and discussions are, together with original communications (not read before the Meetings), and abstracts and extracts of Electrical and Telegraphic science and progress, published at intervals in the form of a journal, of which every Member is entitled to receive one copy.

At these Meetings every Member, Associate, and Student has the privilege of introducing one friend.

NICHOLS AND SONS, 25, PARLIAMENT STREET, WESTMINSTER.

JOURNAL

OF THE

SOCIETY OF TELEGRAPH ENGINEERS.

VOL. III. 1874. No. 8.

The Twenty-fifth Ordinary General Meeting was held on Wednesday, the 25th March, 1874, Mr. Latimer Clark, Vice-President, in the Chair.

The Paper read was—

ON THE DECAY AND PRESERVATION OF TELEGRAPH POLES.

By WILLIAM LANGDON, M.S.T.E.

There is probably no question of greater importance to the Telegraph Engineer than that of the preservation of Telegraph poles.

The earlier Telegraphs were all constructed with the very best Baltic timber. The first line erected for commercial purposes, viz. that between Nine Elms and Gosport upon the London and South-Western Railway, in 1844, was built entirely of the best foreign timber. It was about the year 1851 that Mr. Edwin Clark introduced the use of native-grown larch poles for economical reasons. Square timber is however still largely used for terminal poles, and for positions where sightliness or strength is required; each pole being cut according to its requirements.

All square cut timber is of foreign growth, and that most generally employed is known as Memel. It is cut from the forests of Poland, where it grows in great abundance. Of slow growth, it is close grained. "Swedish" is also frequently used. It is the same wood as Memel, but grown in the country its name indicates. It is generally younger wood and by no means of so strong a nature.

Of Memel we have considerable experience. For over 100 years it has been largely imported into England, and as largely used

where strength and durability have been matters of importance. Within the past few years the demand for it has so increased that the supply has hardly kept pace with it; the natural consequence being an advance in its value.

Pitch Pine (*Pinus rigida*), a distinct species of fir from that known as Memel, growing in abundance throughout the Southern States of America bordering on the Gulf of Mexico, has for the last ten or fifteen years been very largely imported into this country. It was thought to be well fitted for telegraph poles, and to be very durable, from the apparently large quantity of turpentine held within the wood. Our experience of it is not equal to that of Memel, of which, as I have said, we have more than one hundred years' experience, whereas of Pitch Pine we have barely twenty. Yet it is already losing ground for such purposes as we require it. Being a wood grown in a country in which vegetation of every kind revels in the most complete luxuriance, the tree attains gigantic proportions in a very short time. As a natural consequence of this rapid growth it contains a large quantity of sap, but which in this wood is not so easily distinguished from the heart or core wood. A tree of this description will, at the age of thirty or forty years, assume proportions equal to those of Memel wood which has stood its eighty or one hundred winters. Usually imported green, it for a time possesses all its tensile strength, but when exposed to the effects of our atmosphere, particularly when set up on end for a few years, the turpentine held in the wood drains out of it or evaporates, leaving the grain dry and *short*, disqualifying it for fulfilling the duty of a telegraph pole required to withstand a considerable strain. Although apparently not well suited for telegraph poles, it is largely employed for structural decorations, and is yearly coming more and more into use for household furniture.

Memel, Riga, Dantzic, Norway, Swedish, and what is in England known as Scotch fir, are all the same species—*Pinus sylvestris.*—The great difference which exists in the foreign and home-grown wood is simply the result of the difference of climate and soil. The genus is the same.

The pine forests of the north of Europe are the most valuable,

especially on account of the quality of the timber. Once they abounded over the greater part of the continent and the islands, but in the latter situations they have been exhausted. Some century and a half ago there were extensive pine forests in the north of Ireland. In the lowlands and rich soils of Scotland there perhaps never was an extensive pine forest, but there can be little doubt that upon the highlands the pine was once as general as it now is in the back settlements of Upper Canada.

It was about the year 1851, as previously stated, that native grown larch (*Larix communis*) was first introduced for telegraph poles.

A great portion of the beautiful city of Venice is built upon piles formed of this species of timber, which grows abundantly in Italy and Switzerland. It is said to have been introduced into this country in the early part of the seventeenth century, and to have been cultivated as a forest tree early in the eighteenth century. Since that time it has been extensively planted, more especially in Scotland; and its success has been far more uniform, and far greater, than that of any other tree not a native of the country.

This timber when used for telegraph purposes is required to possess the natural butt, as much for durability as for strength. Formerly round poles were usually specified to be not less than 4″ diameter at the top, and for heavy lines 5″. Of later years these dimensions have been increased to 5″ and 6″ respectively. The age of the tree when cut ranges from 25 to 50 years. Its rate of growth varies with the soil. The most durable are the closest grained; and the closest grained are those grown upon poor and chalky ground. The best period for felling them is October or November, when the natural juices or sap cease to rise.

All kinds of wood are cellular and fibrous in their construction. The true woody matter or ligneous fibre consists of *cellulin*, which is the basement tissue of all vegetables, from the soft spongy mushroom that we eat at table to the hard durable oak that forms the table; and *lignin*, which lines the interior of these cellules, giving them strength and hardness. All trees are composed of these two materials, and one tree only differs from another in the

form, compactness, and relative quality of these materials, and in the resinous matters which give them colour. The tree is supported and maintained by the liquids which it absorbs from the earth, the moisture it imbibes from the atmosphere, and the gases it inhales from the air. Every tree is a chemical laboratory, which, in the mastication, assimilation, and conversion of its food into cellulin and lignin, resembles in a marked manner that wonderful operation of the human system which in ourselves produces the concomitant parts, blood, flesh, and bone, which hold together our mortal frames. Slight as is our knowledge of these details, how forcibly do they bring before us that marvellous perfection with which the Almighty has completed His work and provided for the support of every created thing.

When the tree is cut down all the undigested liquids in course of time evaporate and disperse, provided it is so placed as to enable this so called "seasoning" to go on; but if they are allowed to remain they putrify or *ferment.* Moist wood itself although seasoned when exposed to air decomposes and ferments; oxygen is absorbed, carbonic acid and water are given out, and the wood becomes *humus* or *mould,* a little world for lichens, fungi, animalculæ, insects, and numerous specimens of animal and vegetable life. Moisture is the indispensable element of decay, which is favoured by stillness, high temperature, and the absence of light. Hence the destructive effect of damp cellars, where the effect of decay is evident even to our nostrils. Vegetation is the invariable accompaniment of decay. It may be as much its cause as its effect, although it is probably the latter.

Trees even when living do not always retain unimpaired their strength and structure, for we frequently see the interior of old trunks decayed and gone.

There are two kinds of decay, viz.: *dry* and *wet rot.* The former is a slow mouldering action due to the presence of a species of fungus called the *Merulius lachrymans,* by which all the tensile strength and cohesion of the wood is destroyed, and by which it is easily reduced to a fine powder or snuffy dust. To this form of decay we are fortunately so seldom subject that it may be fairly disregarded.

It is in *wet rot* that we find our great enemy, and it is towards this form of decay that our attention should be wholly directed, with a view towards, at the least, allaying it present ravages.

Wet rot is of two kinds, *oxidation* and *disintegration.* The first is a species of slow combustion or "eremacausis." The albuminous and nitrogenous materials of the sap ferment under the influence of heat and moisture, and re-act on the cellulin and lignin which they decompose. Worms, insects, fungi, animalculæ, &c., enter the fibres of the wood and disintegrate its structure by their growth and multiplication. The oxygen of the air unites with the remains, and slowly but surely rots it away.

The opposition which a pole is able to offer to this attack upon its integrity depends very much upon the rapidity of its growth and upon the ground in which it has grown. The slowly growing sturdy larches from the poor hills and rough mountains of the Highlands, that take forty and fifty years to reach maturity, last unimpaired for twelve and fourteen years, but the rapid growing poles of Devonshire, that when grown up only number fifteen to twenty-five years of age, and which are fed by the rich soil of that county, commence to rot in three and four years.

The nature of the ground conduces greatly to the rapidity of this decay; in light sandy soils they go quicker than in thick clay, in chalk than in rock, in vegetable mould than in gravel.

Experience gives the average life of an unprepared telegraph pole as seven years. In many instances, however, this is considerably over the mark. Instances have come under my own observation where the decay has been so rapid as to call for the removal of the pole in a period of three years. Fortunately, this is not general. The durability of the native larch of this country, if grown upon chalky and hard ground and well selected and seasoned before planting, may, as a rule, be regarded as seven years. If, then, we accept this term as the life of a telegraph pole, we may arrive approximately at the commercial value of this question.

The mileage of telegraph lines in England, Scotland, Ireland, and Wales may at the present time be fairly stated to be not less than 24,000. Take it that one-half of these lines are built of

timber protected by some preservative process—there remains 12,000 unprotected. The renewal of these lines at £20 to the mile —a sum no doubt considerably under the actual cost—represents a sum of £240,000.

If we can add to the duration of the life of the timber one-half of the average term (say seven years) it means a saving of £120,000 during every such period. Hence, the preservation of timber is economically and commercially a very large question.

It is well known that the point at which our poles decay, whether planted (as they never should be) in a green state, whether seasoned or otherwise prepared by any process for the preservation of timber, is that at which the greatest strength is required, viz., the ground line, known as the *wind and water line.* Why should it be peculiar

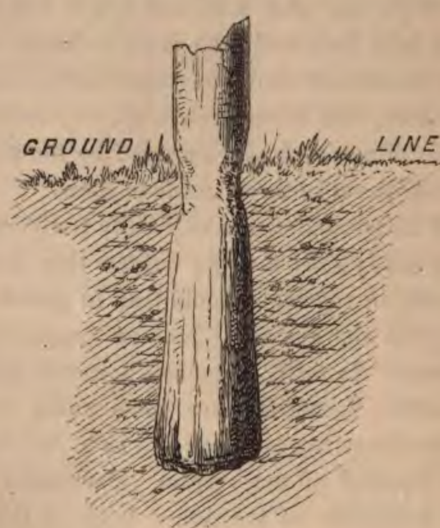


Fig. 1.

to this particular spot? If we take a piece of the same description of wood and completely bury it beneath the soil, or submerge it in water in such a manner that it may remain there undisturbed, we find it last an almost interminable period. Beech has been found sound after having been buried for centuries. In like manner we find wood kept uniformly dry, like the furniture of our houses, last

a very long period; but where timber is subject to a variable temperature, moisture, and exposure to the air, then decay sets in. This we find exemplified in the flooring and skirting-boards of our rooms, and in numerous other instances where the wood on one side is kept almost uniformly dry, while on the other side it is subject to the varying dampness of the foundation.

We find then that wood, when not affected with dry rot, may be said to retain its lasting properties for a prolonged period, under the following conditions:—

1. When kept dry.
2. When continually under water.
3. When wholly buried beneath the soil at such a depth as to secure an equable temperature and moisture.

It may safely be said that in no case do telegraph poles conform to any one of these conditions. The condition of a telegraph pole once planted speedily becomes analogous to that of its surrounding medium. Whilst that portion above ground, if unprotected by paint or some other oily preservative, acquires a degree of moisture almost equal to that held in the atmosphere surrounding it, the portion buried in the soil obtains a moisture even greater than that of the soil by which it is surrounded. Of this everyone may satisfy himself by examining a pole on its removal from a damp soil. The lowest portion—the butt end—will be found saturated with wet, the density of which becomes less and less as the ground line is reached. The moisture contained in the butt end of a pole is due to two causes—viz., absorption from the contiguous ground and percolation from the atmosphere.

I have explained the decay due to eremacausis or slow combustion, but there is another process of disintegration of a mechanical nature, and it is this process to which the destruction of our poles is mainly due.

Every pore of that portion of the pole placed in the ground becomes charged with moisture by absorption, percolation, and capillary action, to a greater or less extent.

If we imagine a sponge placed in a similar position we have an exaggerated illustration of the action to which a piece of timber so situated must, by the force of nature, become subject. The upper

portion of it coming into contact with the warmer atmosphere, the small particles of water held in suspension become heated, vaporized, and finally dispersed into the atmosphere.

Of a precisely similar character is the action to which that portion of the pole level with the ground line becomes subject. The tiny particles of moisture held in the outer pores of the wood first become heated, then volatilised, and, finally bursting asunder the delicate fibres by which they are bound, escape into the surrounding air. No sooner has one particle of moisture burst the bonds of its chamber, than it is followed by another and another; each one aiding in the work of disintegration until its structure is utterly destroyed.

That moisture penetrates from the sides of the pole, as well as from the bottom, although not so rapidly, will scarcely require proving. It may, however, be worth while, taking advantage of this opportunity, to refer to an experiment not long since carried out, with the object of observing the conductive power of timber for moisture in other than the ordinary direction.

It is probably well understood that the system of "Boucherising" means the injection of sulphate of copper, under pressure, into the timber required to be so prepared.

Coarse grown timber is usually selected for this purpose, the Scotch fir being that generally used. The process is most successful during the spring and summer months, when the sap is in the wood. The tree is consequently cut down and drawn while green into the boucherising yard.

As is most natural, the process is applied to the butt end, and, as a rule, almost the moment the solution of sulphate of copper is turned on, the sap will be observed running from the top or small end of the stick. In this process we have a fair means of testing its conductivity of moisture—the pressure being equal in all cases. The experiment had for its object to determine, if it were possible, to treat a pole with equal success from the small or top end as from the larger or butt end. Such proved to be the case. The application of the solution of sulphate of copper at the small end almost instantly started the sap, which poured out from the larger end. The *boucherising* of the pole was completed, moreover, in the usual

time—that is, it occupied no more time to treat the pole in the direction named than it would had the process been applied from the natural or butt end.

That there is also a lateral connection between the fibres may be proved by reference to a creosoted pole. An inspection of the pole will show that the creosote has penetrated from either end some 12 or 18 inches. This is the extent to which the pores of the wood have carried it in that direction, whilst from the sides of the pole it will have penetrated but an inch or two.

From these examples, as well as from the ordinary method of boucherising, we possess proof of the conductibility of moisture in wood in an *upward*, in a *downward*, and in a *lateral* direction, supporting the theory already advanced, by which you are asked to regard the pores of that portion of the pole buried in the soil as so many miniature force-pumps, to which the warmer stratum of air above the ground line acts as the motive power, the action being for the main part from the bottom of the pole upwards, although after a long lapse of dry weather it is possible that on a deluge of wet this action is for a short time reversed; the pores in this case acting simply as pipes to carry the surface saturation to the lower portion of the pole, until the whole is charged alike.

The methods which are adopted for the preservation of timber from the destructive effects of dry and wet rot are of two kinds, the one applied *externally* (A) and the other *internally* (B).

(A). The external applications are :—

1. *Seasoning.*

The tree when felled is cleared of its branches, cut to the required length, barked, the knots shaved down, and the pole then stacked in such a manner that the air can circulate freely around it and favour the escape of the natural juices of the wood. The bark, if left on, harbours boring-worms and insects, confines the sap, and prevents evaporation. When the pole has been so exposed for such a time as to be dry, it is said to be *seasoned.* Timber is sometimes artificially seasoned in hot-air chambers. It is also frequently immersed for a long time in salt water, by which pro-

cess the sap and germs of decay are supposed to be washed out; but the most effective seasoning is that due to exposure to the air. The poles must not however be exposed to the sun or they crack, nor should they be exposed to rain or they will absorb water.

2. *Charring and Tarring.*

Charring consists in slightly roasting or carbonizing the surface of the wood, when it is dry, to a distance of six feet or more from the base of the pole. It expels the sap, if any remain, destroys the external ligneous pores of the wood, coating it with a hard shell, checking absorption, and it kills the germs of animal and vegetable life that exist in that portion of the tree. It is done by placing the pole, for the length required to be charred, over a moderate fire, and removing it as soon as the surface becomes black and before it catches fire. The portion charred and some three feet of the pole above is usually coated with tar while it is hot.

When a pole has been not merely charred, but also well tarred about the wind and water line only, we notice that decay is removed to the point *above* that so treated, at once giving evidence of the good done, and suggesting that, if the whole of that portion of the pole placed underground had been served in a similar manner, the destructive action might have been still further delayed. Efforts seem to have been directed almost solely to that point at which decay has presented itself. The bottom of the pole is frequently neither charred nor tarred. Its lower portion is thus left unprotected, and open to the absorbing action. It is here that moisture creeps in, and it is from this point that, under the influence of the higher temperature above, it is drawn upwards, until it reaches that part where the pores of the wood are no longer sealed up. Here it breaks forth, tearing the fibres asunder, disintegrating and destroying the wood in the manner observed.

It is important in charring that the wood should not be *burnt.* If it be so, the tar will be absorbed by the burnt portion, and will scarcely penetrate beyond it. In planting the pole the charred portion will probably become detached, and the object will thus be defeated. When a pole is burnt it is better to scrape off the burnt portion before the tar is applied.

The tops of the poles, if not preserved from moisture, decay. In some countries they merely point the poles and paint them, but in England they are invariably protected with roofs and caps, usually galvanized iron roofs bent and nailed on the top, sloped for the purpose.

3. *Local Applications.*

The general decay of telegraph poles began to develop itself in the year 1855, and, its theory not being fully understood, local applications to the "wind and water" mark were applied. Gangs were sent about the country to remove the ground to a depth of about 2 feet, cutting off the decayed part of the poles, and plastering it above and below the ground line with asphalte, and eventually with cast-iron and earthenware cylinders filled with asphalte (Fig. 2). But these and other plans applied to the same part entirely failed.

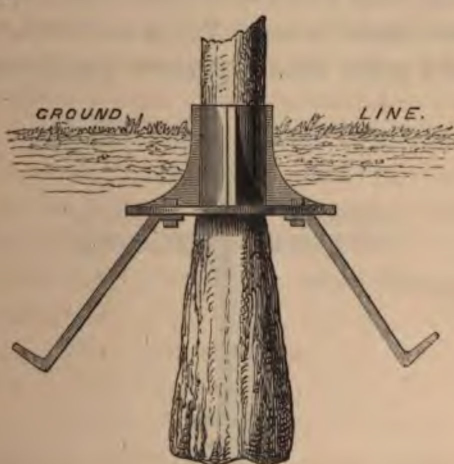


Fig. 2.

Some years since Mr. Preece attempted to stay its ravages by encasing the butt-end of the pole in an earthenware jar (Fig. 3), filling up the space between the jar and the pole by a compound of hot tar, lime, and felt; but the experiment was not successful. Its failure was possibly due to want of care in carrying out. It would moreover appear, that, to be successful, the entire portion of the pole buried in the soil should be rendered waterproof.

During the discussion on telegraph poles in the early portion of the past session, a letter from Mr. Haynes, the telegraph super-

Fig. 3.

intendent of the Bristol and Exeter Railway, was read, in which it was stated, that, about some twenty years since, the Electric Telegraph Company erected between Yatton and Worle station three miles of wooden poles, fitted with screw iron sockets. (Fig. 4.)

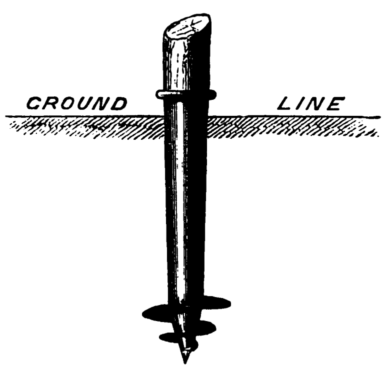


Fig. 4.

Mr. Haynes then proceeds to state: "I cannot learn that any renewals were necessary until last year. I then found that several of the poles had gone badly near the sockets, and others were rotted near the top. Those that were bad at the bottom were several inches below ground, and had evidently been in that position for a considerable time. They apparently had either

forced the sockets deeper into the ground, or the sockets were buried too deep in the first instance."

It is of course equally possible for the poles thus referred to as being buried too far in the soil to have had the soil raised around them by fresh ballasting. Be that as it may, we have here a strong argument in favour of the theory advanced. These poles have stood twenty years, and of those showing signs of decay the decayed portion is that which has been exposed to the soil.

(B.) The internal application of the best known preservative processes is of two kinds:—

1st. The introduction into the pores of the wood of some *salt*, which, uniting chemically with the albumen of the sap, is stated to convert it into an insoluble compound;

2nd. The introduction of some *oil*, which not only acts as an antiseptic, but renders the woody tissue waterproof.

Under the first head several processes have been tried, but the best known are Burnetising, Kyanising, and Boucherising.

Burnetising consists in impregnating the timber when dry with a solution of chloride of zinc. The original poles on the London and South Western Railway erected in 1844 were so served, as were others erected on the Midland in 1846. They were in both cases of the best Memel timber. The process does not appear to have been much employed of late years.

In 1857 Mr. Warwick, reporting upon the condition of Burnetised timber, found, out of 17,

4 in good condition,
12 slightly decayed,
1 bad;

the poles having been standing for about eleven years.

Mr. Preece shortly afterwards, in reporting upon an examination of some 47 miles of line, found the proportion of decayed poles —poles which were become so bad as to require spurring—to be as follows:—

In sand 2 in 5
In clay 2 in 6
In chalk 2 in 7.

The poles having been planted in this case 13 years.

Three years later Mr. Preece again reports as follows :—

In sand	74	out of	97
In chalk	50	out of	112
In gravel	25	out of	70
In clay	18	out of	73.

The age of these poles being at that time 16 years. All these poles were renewed by the end of 1871.

Kyanising consists in impregnating the timber with a solution of corrosive sublimate (mercuric chloride).

In both Kyanising and Burnetising open tanks are filled with the solution with which the timber is required to be charged. The timber is then submerged in it, well fixed down, and allowed to remain soaking until it has absorbed the proper quantity of the solution.

On the Cornwall Railway some 400 larch poles, prepared under this process, were erected some seven years since. Mr. Webber, the Telegraph Superintendent of that line, states they are apparently as sound as ever.

Boucherising consists of impregnating the timber with sulphate of copper. This is effected by applying a solution of this description to the butt-end of the pole under a gentle pressure provided by the weight of the liquid itself, which is usually arranged in tanks at a height of some fifty feet above the level at which the timber is placed. The lasting properties which this process imparts to timber appear to vary considerably. During the discussion previously referred to (Iron poles, Session 1873) Mr. Culley pronounced it a failure under special circumstances. In some instances poles which have not been planted more than six years have heen removed owing to dry rot. In other cases they have still been found good after standing 13 or 14 years.

The Yeovil and Exeter section of the London and South Western Railway was built of poles prepared under this process in 1861. The line was wholly renewed in 1871, prior to which date very many poles had been replaced. It is however only fair to say that at the time of the renewal some few poles still remained sound, and are still in use on another section of line.

Boucherising has, in spite of its apparently uncertain action,

the recommendation that it can be applied in the wood in which the tree grows, thereby probably saving a considerable outlay in carriage; that poles prepared by it can be made use of immediately; in fact that a tree may be cut down, hauled into the Boucherising yard, subjected to the process, and erected as a telegraph pole in a month, and this with a prospect of its lasting some 10 or 12 years.

With other processes of this character this is not so. With the Kyanising or Burnetising processes it is necessary that the wood should first be thoroughly dry.

Attempts have been made to render timber indestructible by supplying the tree with antiseptic salts while growing. The most

Fig. 5.

complete experiments in this direction were made by Mr. Hyett, F.R.S., who not only supplied the living tree with preservative

materials, but changed the colour of the wood so as to make beech appear a beautiful mahogany, and larch an exquisite rosewood.

About 6 inches from the ground a 2-inch auger-hole was bored quite through the centre of the tree, a narrow saw was then introduced, making a horizontal cut right and left of the tree. A clay basin holding about two gallons was arranged around the bottom, in which the solution was placed. In this way, it is stated, trees became thoroughly impregnated with the required salt.

We now come to the preservation of timber by oil, and under this head we have simply to deal with that system known as *creosoting*. The theory of the application of creosote is that it is not only an antiseptic, but it destroys any vegetable germ which may exist in the timber, and that it renders it impervious to moisture. Its usul mode of application is by placing the poles within a cylinder, which is then made air-tight, and the air exhausted from it. In exhausting the air it is said any moisture held in the wood is also removed. This is very doubtful. However, the air being exhausted, the creosote is then turned in the cylinder, and injected into the wood, under a pressure varying according to the contents of the cylinder.

There can be no question that in creosote we have by far the best preservative yet produced. It is cheap, easily applied, and effective.

Perhaps no greater recommendation of its efficiency can be offered than the fact that there is now standing, to all appearance as good as when first erected, a line of poles between Fareham and Portsmouth on the London and South Western Railway which was erected in 1848-9. These poles are now five-and-twenty years old, and, when examined a few months since, gave no sign of decay.

In 1861 Mr. Preece, reporting upon this form of preservative, states that out of 318 poles examined only two gave signs of decay, and that at the top of the poles. The poles had been standing twelve years. The next line of poles of creosoted timber planted on the London and South Western Railway was that between Epsom and Leatherhead. They were erected in 1861, and are still apparently as good as when first put in. Very nearly the whole of the London and South Western Railway Company's Telegraph system is now constructed of creosoted timber.

In creosote we no longer deal with an *absorbent of moisture*, but with something *directly opposed to it*. Hence its power of resisting that action which has been described as destructive to the pole, and hence the durability of creosote over all other descriptions of prepared timber. Let the timber be dry when the creosote is applied, and let it be applied under proper pressure; the result need not be questioned. But let the timber be wet when subjected to the process, and it is probable that at no distant date the core of the wood, or that portion of it between the heart and the point to which the creosote has penetrated, will become rotten; for to this point will have been driven the moisture contained in the wood. Here it must remain, for there is no escape through the creosote. It must consequently ferment, rot, and destroy the core of the pole.

In employing creosoted timber care should be taken when the creosoted portion is removed, as is sometimes the case in fixing the tie-pieces at the foot of A poles, that the uncreosoted portion exposed be well tarred, and even where the same occurs aboveground—as when slotting poles for arms—that it should in some way be preserved from the effects of wet. No great time since, when Railway Companies began to make use of creosoted sleepers, it was not an unusual thing to see them fitted for the chairs after they had been submitted to the process. The natural consequence was that the point at which the greatest durability was required was the first to decay. We now see sleepers fitted before being creosoted. As yet we have small experience of the final durability of creosoted poles, and, although hitherto the tops of our poles have been but a secondary consideration in consequence of the certain more rapid decay of the lower portion, such may not be the case in future.

Neither should it be forgotten that, whilst creosoted timber is impervious to wet, it is not so to the sun's rays. A creosoted pole will always show on which side the sun shines. The oil is by his influence brought to the surface of the wood. Some of it evaporates whilst the rest trickles down the pole, forming a little mass around its base at the ground-line. It is desirable for this reason that such poles should occasionally be served with a coat of tar.

As to the period at which this should be done, the pole itself will be the best guide. Poles treated under this process differ considerably in the manner and measure in which they accept it. Their dryness, growth, and texture are all circumstances by which their absorption of the oil is governed. A damp, or a close-grained, pole will not receive it so well as a dry or coarse-grained pole. As soon as the pole assumes a light rusty brown colour then it is desirable to give it a coat of tar,

The great expense which attends the carriage of timber renders it scarcely possible to obtain creosoted poles in all parts. Were it otherwise, however, other objections accompany its employment. To some, black poles are an eyesore, whilst to others their smell is objectionable. We have seen that the decay of our poles takes place from the butt-end upwards, to something from twelve inches to two feet above the ground-line, and, if the cause of this decay is that suggested in this paper, it would seem that where a difficulty attends the provision or use of poles wholly creosoted, the preservation of the timber might be secured by creosoting the bottom portion only.

Fig. 6.

In 1870-71 a new line of poles was erected by the Postal Telegraph Department between London and Beachy Head. It was

thought desirable that some of the poles used should be prepared after this fashion. A large tank was obtained and arranged so that a fire could be lit underneath it. In the tank was placed a quantity of creosote, which was kept at boiling heat, and the butts of the poles were submerged in it, as shown in the diagram (Fig. 6). A man in attendance looked to the fire and kept constantly laving the poles so as to secure an application of the process for six or eight feet of the butt-end of the pole. By this means some twelve or fourteen poles at a time received a very fair surface-coating of creosote, which, although not penetrating so far as would have been the case had they been treated under pressure, was yet very superior to a coat of tar, the wood being penetrated from a quarter to three-eighths of an inch.

This very modified system of creosoting is no doubt capable of improvement. It has advantages of great moment. The necessary apparatus for its application can be mounted upon wheels. The tank can assume the shape of a wagon, and it can be fitted with a regular heating arrangement. In this way it could with convenience be moved from place to place—set up in the very wood in which the timber is cut, trimmed, and stacked. It leaves the upper portion of the pole free for painting or other treatment, and it *in no way impairs the elasticity of the wood.* It would, of course, be wholly premature to attempt to draw conclusions from the present condition of the poles on the Beachy Head line served under this process, unless it was of a negative character. It may be mentioned, however, that a recent examination of them has been so far satisfactory, the timber being to all appearance as good as when first planted.

It would appear very desirable that, in all cases except where the whole of the pole is regularly creosoted, the butt-end should be served with a good coating of tar before being placed in the ground. And this applies to poles prepared under a salt as well as to plain poles. If there is any virtue in infusing into the wood a solution of such a description, it would certainly appear proper to provide against its escape. That poles prepared under this system do lose some portion of the salt injected into them may be seen, on examining the soil in which a boucherised pole has been planted.

For some twelve inches or more around the pole the ground will be found tainted with sulphate of copper. With a chalky soil the discoloration is very perceptible. This will probably be best prevented by serving the butt-end in the manner named, taking care to apply the tar to the bottom of the pole as well as to the sides.

In conclusion then we may gather from the foregoing :—

1. That creosoted poles planted twenty-five or twenty-six years since are still apparently in a state of perfect preservation.
2. That poles fitted with iron sockets which exclude the moisture from the wood, planted twenty years since, are still in a good state of preservation.
3. That specially selected foreign timber, Burnetised, has lasted with repairs from eighteen to twenty-four years.
4. That Boucherised timber may be said to last ten to fourteen years.

It would thus appear that, with those systems classed under the head of preservation by a salt by which *moisture is contracted*, we have a varying and uncertain extension of the life of timber; but in no case is this extension such as to compete with that secured by creosote, by which *moisture is rejected.*

It should be observed, that all timber when treated by any of the preservative processes in general use becomes, under certain conditions, what is technically termed "*short*," that is, it easily breaks in two. When the whole of the wood is thoroughly impregnated with the process, its tensile strength becomes impaired. It no longer, when dry, retains the same amount of elasticity it possessed when in its natural state. Timber treated with a salt recovers a great deal of this when in a moist state, as it generally is when in the ground. Creosoted poles do not hold moisture, but it is seldom the oil penetrates the whole of the wood, consequently there is still some portion of its elasticity left This shortness may be easily tested by taking a piece of dry creosoted or other preserved timber, and trying to split it with an axe. It will be found that the axe will not follow the course of the grain of the wood.

It is with very much pleasure I acknowledge the kind assistance

which Mr. W. H. Preece has rendered me in placing at my disposal information, and otherwise directing my efforts in such a manner as has, I trust, enabled me to render this paper more complete and valuable than would otherwise have been in my power.

DISCUSSION.

The author of the paper described in detail the various specimens of timber exhibited in illustration of the views he had put forward.

The CHAIRMAN: In inviting discussion upon this paper I would remark, as the author has done before me, that the subject is one of great importance, both as to the state of things in this country and in foreign countries. I therefore hope members who can give information upon it, however slight, will not fail to do so. We shall now be glad to hear any remarks that gentlemen may favour us with.

Mr. GOLDSTONE (responding to the Chairman's invitation) said: I do not know that I can say much on this subject. I have been talking this matter over with Mr. Langdon, and compared our views, in which we are very much agreed. I would just advert to the drawing No. 6. The first timber I ever saw prepared in that way was some hop-poles in Kent. I thought a wrinkle might be got from it. Going over some hop-grounds, a man showed me a lot of poles which he had served in this way. They were placed in a tank of boiling creosote, and it was stated to me that a considerable saving per annum was effected through this simple treatment of the poles. It occurred to me that, if it was beneficial for hop-poles, it would be a good thing for telegraph poles. About twelve months after that I met Mr. Langdon, and he suggested to me to serve some telegraph poles in that fashion. I did so, and the poles have been put down, but the time as yet is not sufficient to prove whether that really does the required thing or not. I hope on the next occasion to bring you a piece of one of the poles which Mr. Langdon referred to as having been in the ground for 25 years. I had reserved with this object a pole that was taken out as being too

short for its purpose, and which was put by in case it was required for any other work; but I find it has been used notwithstanding; but I will have one here at the next meeting.

The CHAIRMAN: Is that a creosoted pole?

Mr. GOLDSTONE: Yes. The boucherised poles were used on the South Western line. I rather like the boucherised poles, but I cannot say it really answers the purpose so well as creosoting, but boucherised poles can be painted and made to look prettier. It is useful to be able to put in a piece of timber which is as easily adapted as a piece of plain timber. The Yeovil and Exeter line was taken up in 1870. It was planted by the old Telegraph Company as an experimental line. At the time it was planted they put in alternately plain poles, creosoted poles, and boucherised poles, in order to give each kind a fair trial. The unprepared poles were larch, Scotch fir, and spruce; the boucherised poles were all Scotch fir; and the creosoted generally Norway red fir. Not one of the creosoted poles was found to be decayed, and almost without exception they were used again; but not one of the plain poles could be used again—all were more or less rotten, and some of them in a very bad state indeed; the majority of them were more or less rotten all the way up, especially up to the ground-line. Of the boucherised poles we found that about 30 per cent. of them were gone, very much in the way you see these here, just about the ground-line. It occurred to me the reason for that was, in some cases they had not been properly boucherised. We were obliged to plant the whole line afresh, and as the wires were increased in number the poles were shortened. A great many of the sound boucherised poles were used to renew the other line; they are still in use, and in time we shall know the real value of them.

Mr. BELL: From my experience, I believe the charring and tarring treatment is rather detrimental than otherwise to the poles, unless they are quite dry; as in the case of creosoted poles, unless quite dry, the moisture is driven inwards. I knew a case, on the York and North Midland Railway, in which larch poles were felled and quickly treated by this process, and erected before they had time to season. The result of that was that after four years the poles had to be repaired by spurring. A great portion of the poles

were completely decayed; but it was a singular fact that where the charring commenced the decay took place; the sap of the poles had descended to that spot, and there it had stopped, and there the decay was commenced by the moisture being left in the pole. I was so much struck with that fact, that, as I had some lines to erect on branches of the South Western Railway, I proposed that the top of the holes should be left open, so that the wood might get seasoned. Of these poles some were boucherised; but they were mixed, and a great many were larch poles put up without preparation; but the holes were left open with the intention of filling them up, perhaps, a year or so afterwards. I do not know what was the success or otherwise of the experiment. Respecting the mode of preparing by tanking the butts, I think that, unless the poles are thoroughly seasoned before creosoting, the treatment would be mischievous rather than otherwise. I have referred to the tar process, and I fear the creosoting in tanks would have a similar effect, unless the wood is seasoned. You cannot hope by tanking to reach beyond a small depth into the wood, while creosoting is done in cylinders under a pressure of 100 or 150 lbs. to the square inch, which forces through the pores of the sap-wood, because it never goes through the heart or red wood. So that I fear a large portion of the sap-wood, which you could not reach in this way, would ultimately decay. There is one process which Mr. Langdon has not mentioned—that is *Beer's* process, in which borax is used. The wood is steeped in it, and it is supposed to neutralise the decaying vegetable matter in the wood, and this was afterwards washed out.

The CHAIRMAN: I would ask Mr. Bell, or any other gentleman present, whether he or they have ever met with a case in which wood creosoted to any depth has ever decayed, or whether they have known of any heart-wood becoming rotten while the outside remained sound?

Mr. BELL: Speaking for myself, I have never seen any creosoted timber decayed, although in the case of railway sleepers the wood has been disintegrated by the action of the chairs. I should like to ask what was the proportion of poles decayed on the Portsmouth line, and whether they were red wood or white wood?

Mr. LANGDON replied they were foreign—Memel, all of them.

The CHAIRMAN asked what portion of the Salisbury and Andover line Mr. Bell was engaged upon.

Mr. BELL: From Bishopstoke to Salisbury and from Basingstoke to Andover. Some of the poles on the latter line, I believe, were boucherised and some were quite unpreserved. Some were almost green timber; they were not hard-grown larch.

Mr. ISHERWOOD (responding to the Chairman's call) said: My experience in timber for telegraph poles is limited, for all I have fixed in London have not been down more than about four years, and they have had little opportunity of decaying. The poles I have fixed in the suburbs of London were some of them treated with the preservative process, whilst some were used without any preparation whatever, but none have as yet shown signs of decay.

Mr. VON FISCHER TREUENFELD: I have had no experience in lines of wooden poles in England, and I can only call attention to some descriptions of tropical timber, which will perhaps be of no advantage to this discussion; but it may be interesting to know that there are tropical trees which will last, I should say, for two hundred years without showing the slightest signs of decay. On one occasion I took up a number of poles which had been used—not for telegraph poles but for building purposes—which had been in the ground for over one hundred years, and they did not show the slightest signs of decay or corrosion. There is a timber in the interior of South America called native lime or iron-wood. That wood is so hard it is impossible to drive a nail into it, and I believe that would remain sound for hundreds of years if it could be brought over to this country and used as telegraph poles. I have myself used it for that purpose.

The further discussion of the paper was adjourned until the next meeting.

The following Candidates were balloted for and declared duly elected :—

As Foreign Member :—

Otto Straube . . .	Buenos Ayres.

As Member :—

Edward Lushington . .	Brackenhurst, Cobham.

As Associates :—

James Bishop . . .	Shanghai.
Edward Lawson . . .	Funchal, Madeira.
James Mathieson . .	Silvertown.
George Ward . . .	St. Pierre, Newfoundland.
William Warren . .	George Town, Tasmania.

As Students :—

W. H. Cochrane . . .	King's College.
Edward Hesketh . .	do.
José Pena	do.

The Meeting then adjourned.

The Twenty-sixth Ordinary General Meeting, held on Wednesday, April 22nd, 1874, Sir William Thomson, President, in the Chair.

The following Paper was communicated by Sir William Thomson, "On Deep-Sea Sounding by Pianoforte Wire."

On the 29th of June, 1872, I sounded, from the "Lalla Rookh" schooner-yacht, in the Bay of Biscay, with a lead weight of 30lbs., hung by 19 fathoms of cod-line from another lead weight of 4lbs. attached to one end of a three-mile coil, made up of lengths of pianoforte wire spliced together and wound on a light wheel, about a fathom in circumference, made of tinned iron plate. The weight was allowed to run directly from the sounding-wheel into the sea, and a resistance exceeding the weight in water of the length of the wire actually submerged at each instant was applied tangentially to the circumference of the wheel, by the friction of a cord wound round a groove in the circumference, and kept suitably tightened by a weight. My position at the time was considerably nearer the north coast of Spain than a point where the chart shows a depth of 2,600 fathoms, the greatest depth previously marked on the charts of the Bay of Biscay. When from 2,000 fathoms to 2,500 fathoms were running off the wheel, I began to have some misgivings as to the accuracy of my estimations of weights and application of resistance to the sounding-wheel. But, after a minute or two more, during which I was feeling more and more anxious, the wheel suddenly stopped revolving, as I had expected it to do a good deal sooner. The impression on the men engaged was that something had broken; and nobody on board except myself had, I believe, the slightest faith that the bottom had been reached. The wire was then hauled up by four or five men pulling on an endless rope round a groove on one side of the wheel's circumference. After about 1,000 fathoms of wire had been got in, the wheel began to show signs of distress. I then perceived, for the first time, (and I felt much ashamed that

I had not perceived it sooner,) that every turn of wire under a pull of 50 lbs. must press the wheel on the two sides of any diameter with opposing forces of 100 lbs., and that therefore 2,240 turns, with an average pull on the wire of 50 lbs., must press the wheel together with a force of 100 tons, or else something must give way. In fact the wheel did give way, and its yielding went on to such an extent that when 500 fathoms of wire were still out the endless cord which had been used for hauling would no longer work on its groove. The remaining 500 fathoms and the 30 lbs. sinker were got in with great difficulty by one man working at a time in an awkward position over the vessel's side, turning the wheel slowly round by a handle. I was in the greatest anxiety, expecting at any moment to see the wheel get so badly out of shape that it would be impossible to carry it round in its frame, and I half expected to see it collapse altogether and cause a break of the wire. Neither accident happened, and, to our great relief, the end of the wire came above water, when instantly the 19 fathoms of cod-line were taken in hand and the 30 lb. sinker hauled on board. I scarcely think any one but myself believed the bottom had been reached until the brass tube with valve was unscrewed from the sinker and showed an abundant specimen of soft grey ooze. The length of wire and cod-line which had been paid out was within a few fathoms of being exactly 2,700 fathoms. The wire was so nearly vertical that the whole length of line out cannot have exceeded the true depth by more than a few fathoms. The position was accurately determined by two Sumner lines observed at 11h. 23m. a.m. and 1h. 5m. p.m. Greenwich apparent time, and found by their intersection to be latitude 44° 32′, longitude 5° 43′ west.

That one trial was quite enough to show that the difficulties which had seemed to make the idea of sounding by wire a mere impracticable piece of theory have been altogether got over.

The great merit of wire compared with rope is the smallness of the area and the smoothness of the surface which the wire presents, in contrast with the greatness of the surface and its roughness when rope with a comparable degree of strength is used. The wire that I

have found suitable is pianoforte wire of the Birmingham gauge No. 22. It weighs about 14½ lbs. to one nautical mile, and bears from 230 lbs. to 240 lbs. without breaking. The quality of wire which I described to the meeting of the British Association at Brighton was special wire made for the purpose by Messrs. Johnson, the celebrated wire-makers of Manchester. They succeeded in producing a length of crucible steel wire of three miles in one piece, which certainly was a great feat in the way of wire-making. This wire was supplied by them to me as capable of bearing a pull of about 230 lbs. I tested many specimens of it, and I found that none of them broke with a less pull than about 220 lbs., and many of them bore as much as 240 lbs. The wire then fulfilled all that the makers promised, and it had that quality which then seemed of paramount importance—a great length in one piece of metal. The truth is, that one of the supposed "impossibilities" was safe splices. However, splices must be made: and in my first trials I succeeded by making a long twist of two pieces of wire together, and running solder all along the interstices. On testing this splice, I found that, although it would bear within 10 lbs. or 20 lbs. of the full breaking-weight of the wire, yet in every case the wire broke at the splice. This was precisely in accordance with theory. The sudden change of area of section between the long cylindrical wire, and the thickening produced by the solder, is an essential element of weakness, of a character well known to engineers. Inevitably, if the wire is of uniform character, it breaks close beside the solder. To avoid this weakening of the wire, an exceedingly gradual commencement of the force by which one piece of wire pulls the other must be attained. The obvious way of attaining this is by a very long splice. A splice of two feet long I have found quite sufficient; but three feet may be safer. The two pieces of wire to be spliced are first prepared by warming them slightly and melting on a coating of marine glue to promote surface friction. About three feet of the ends so prepared are laid together and held between finger and thumb at the middle of the portions thus overlapping. Then the free foot and a half of wire on one side is bent close along the other

in a long spiral, with a lay of about one turn per inch, and the same is done for the free foot and a-half on the other side. The ends are then served round firmly with twine, and the splice is complete. I have tested scores of splices made in this way, and in no one instance, even with splices only one foot long, did the wire break in the splice or near to it. It always broke some distance away, showing that the wire close to the splice was as strong as other parts of the wire, and of course in the splice itself the two wires together give a greater strength than exists anywhere else. In upwards of one hundred soundings on the East and North coasts of Brazil, in the Pacific, and in the Bay of Biscay, in depths of from 500 to 2,700 fathoms, partly with Johnson's special wire, and partly with Webster and Horsfall's, there has in no one instance been a failure of the splice. The splice is made very easily, and in a few minutes.

The difficulty with regard to splices being altogether got over, we are freer in our choice of the wire to be used. Mr. Johnson tells me that it is impossible to produce in the great lengths the same quality of wire as is habitually made by the best makers of pianoforte wire. He said that, although he could produce wire of great strength, he found it impossible to attain the same temper as that of the pianoforte wire. Acting upon his valuable advice, I have now begun to use pianoforte wire of the best quality. Wire of an inferior quality is brittle at places, and breaks when it kinks. I believe not a single case of this has happened with the Webster and Horsfall pianoforte wire now used.

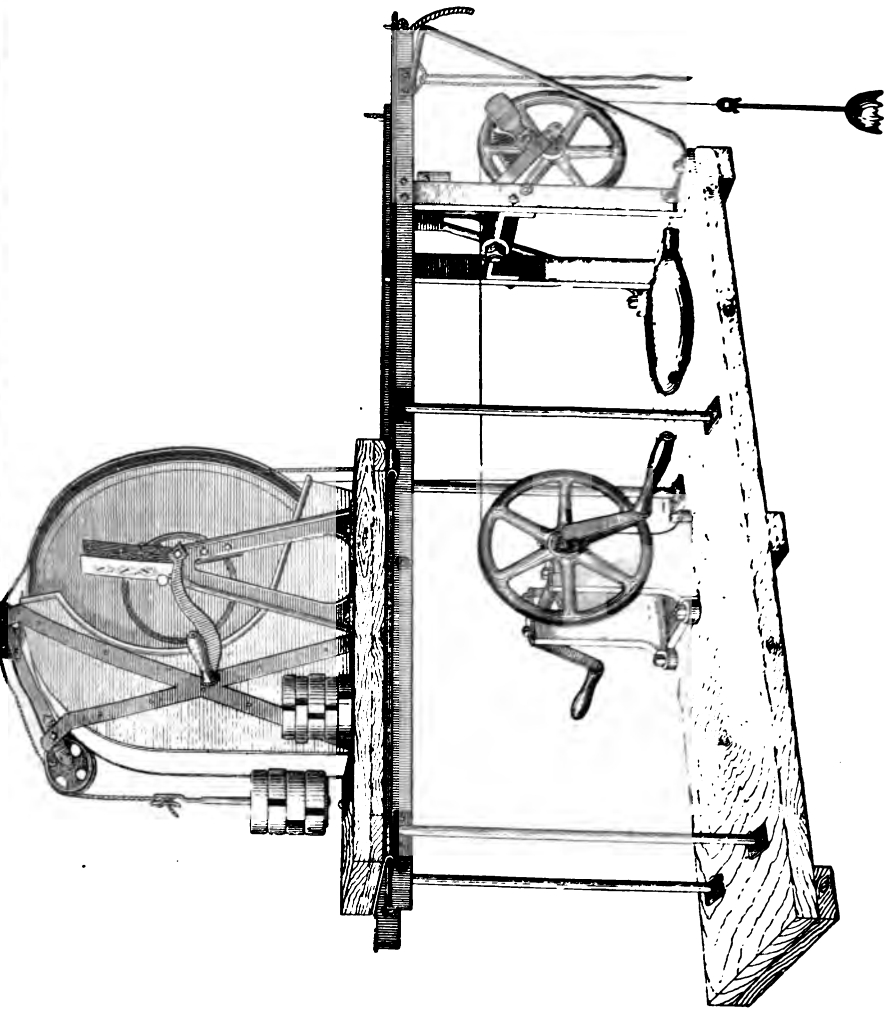
The lengths which Webster and Horsfall supply of this wire are about 200 yards But a splice in every hundred fathoms is no inconvenience whatever. Perhaps it is rather an advantage: because, practically, the vigilance required to prevent accident through the stripping of a splice by any sharp obstacle is apt to flag dangerously if the passage of a splice is a rare occurrence.

The most serious defect of the simple apparatus which I used in my first deep-sea sounding in the Bay of Biscay was the destructive stress experienced by the wheel in hauling in the wire.

My first attempt to remedy this defect was a failure. It con-

sisted in stopping the hauling every twenty turns, taking the strain off the wire by aid of a clamp, and easing it round the wheel. This was done in a sounding of 1,200 fathoms, made in Funchal Bay, Madeira, only a few miles from Funchal, during the Hooper cable expedition to Brazil last summer. I found that stopping every twenty turns did not seem to be of any use at all, so I stopped every ten turns, and even that tedious process did not afford sufficient relief. That plan having proved a failure, I then looked out for some other; and the peculiarity of the apparatus now before you consists in the way in which the difficulty was overcome. In the American Navy another mode of getting over it has been followed: the wheel has been strengthened, and a trigger apparatus has been introduced for detaching the weight when it reaches the bottom. This of course very much lightens the pull in hauling in the wire. By those means—the strengthening of the wheel and the lightening of the pull—the Americans got over the difficulty very well. I, however, did not consider it desirable to throw away 30 lbs. or 35 lbs. of lead at every sounding, as I believed I could modify the apparatus so as to make it easy to bring up the sinker from any depth not exceeding 3,000 or 3,500 fathoms in ordinarily favourable circumstances; and I wished to reserve the expedient of detaching the weight for greater depths or less favourable circumstances. In case of very great depths, 4,000 fathoms or more, it will probably be desirable to use a heavier sinker, say 100 lbs., and a trigger apparatus for detaching it when it reaches the bottom. But for depths not exceeding 3,000 fathoms, I prefer generally a 30 lb. or 35 lb. sinker, with no detaching apparatus.

The way in which I have got over the difficulty of saving the main sounding-wheel from destruction or damage by the pressure of the wire coiled on it, under heavy pull, consists in the use of an auxiliary hauling-in pulley, by which the pull on the wire is very much reduced before it is coiled on the main sounding-wheel. As in my original process in the Bay of Biscay, during the descent of the sinker the wire runs direct down into the sea from the main sounding-wheel, which, for that part of the process, is placed in an overhanging position on either side of the ship, or over her taffrail;



the taffrail, suppose, to avoid circumlocutions. To prepare for hauling-in, a spun-yarn stopper, attached to the lower framing of the sounding machine projecting over the taffrail, or to the taffrail itself, is applied to the wire hanging down below, to hold the wire up and relieve the wheel from the necessity of performing that duty: or otherwise, two men, with thick leather gloves, can easily hold the wire up.* A little of the wire is then paid out from the wheel; the wheel with its framing is run inboard about five feet on slides which carry its framing [see the accompanying drawings†]; and the slack wire, as shown in the perspective view, is led over a quarter circumference of the "castor pulley," and three-quarters, or once and three-quarters, round the "auxiliary pulley." This pulley overhangs the bearings of its own axle, so as to allow the loop or the two loops of the wire to be laid on it. Two handles attached to the shaft of the auxiliary pulley, worked by one man on each or two men on each, take from two-thirds to nine-tenths of the strain off the wire before it reaches the main sounding-wheel, on which it is coiled by one man or two men working on handles attached to its shaft.

If the ship is hove-to when the wire is being hauled in, the wire will generally stream to one side (if out by the stern, which is the position I now prefer). By having the bearing of the stern pulley, an oblique fork turning round a horizontal axis (like the *castor* of a piece of furniture laid on its side), the wire is hauled in with ease, though streaming to either side, at any angle. [See the drawings.‡] This castor arrangement is a very important addition to the

* The spun-yarn stopper is to be seen in the accompanying perspective drawing, shown as hanging ready for use.

† The side elevation (p. 214) shows the sounding-wheel projecting over the taffrail in the position for paying out the wire: and the perspective drawing (p. 211) shows it as run inboard in the position for hauling up.

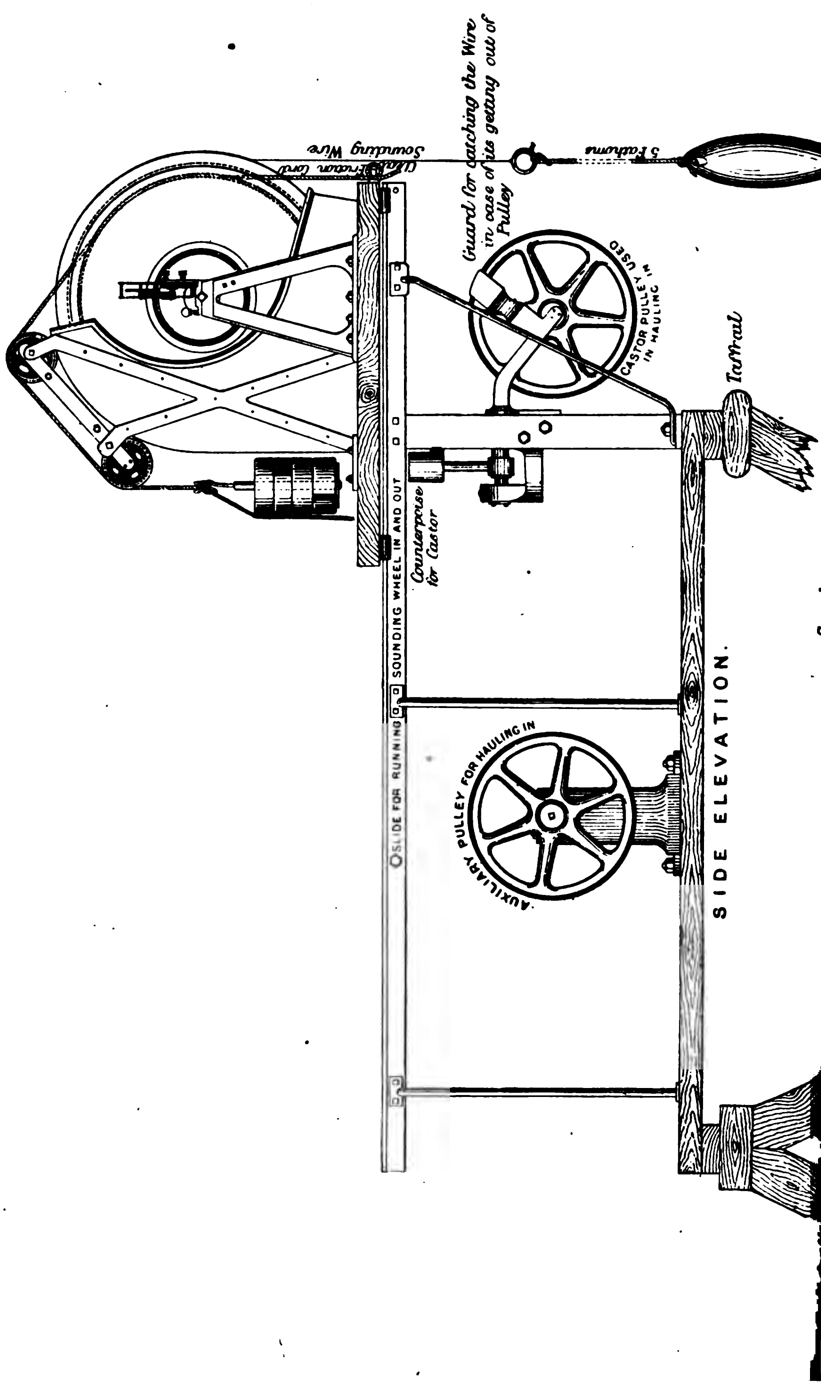
‡ In respect to the arrangement of framing for bearing the castor-axle of the forked piece in which the castor-wheel or pulley runs, the side elevation (p. 214) shows a design of improvement on the arrangement represented in the perspective drawing (p. 211). The improvement consists merely in lengthening the castor-axle, and providing for it two bearings, instead of its having only one, as was the case in the machine shown at the meeting, and as is exhibited in the perspective from a photograph of which the annexed drawing was taken.

hauling-in gear. By means of it it is easy to keep the wire on the stern pulley when the ship is rolling very heavily. Even on the steam-launch of the *Hooper*, rolling sharply through great angles off Funchal Bay, a small castor-pulley which I used accommodated itself perfectly to the motion, and allowed the wire to be coiled safely on the sounding-wheel, which would have been scarcely possible without the aid of some such appliance. The quickness with which the wire allows the sinker to descend, and the ease of getting it on board again by aid of the castor-pulley, notwithstanding a considerable degree of lateral drifting of the ship, render it easy to take deep-sea soundings of 2,000 or 3,000 fathoms from a sailing vessel hove-to in moderate weather.

But it is not necessary to keep the ship hove-to during the whole time of hauling in the wire. When the depth exceeds 3,000 fathoms, it will, no doubt, be generally found convenient to keep the ship hove-to until a few hundred fathoms of the wire have been brought on board. When the length out does not exceed 2,500 fathoms, the ship may be driven ahead slowly, with gradually increasing speed. When the length of wire out does not exceed 1,500 fathoms, the ship may be safely driven ahead at five or six knots. The last 500 fathoms may be got on board with ease and safety though the ship is going ahead at ten or twelve knots. Thus, by the use of wire, a great saving of time is effected; for in the ordinary process the hemp rope must be kept as nearly as possible up and down, until the whole length out does not exceed a few hundred fathoms.

[Sir William Thomson next proceeded to explain in detail and to exhibit in action a new sounding-machine which had been made according to his designs by Mr. White of Glasgow for Messrs. Siemens, to be used on board their cable ship *Faraday*, and which, through their kindness, was brought before the Society this evening. The machine is represented in the present report* by a side elevation and a perspective drawing made from a photograph of the appa-

* More detailed drawings are published in the Proceedings of the Philosophical Society of Glasgow for Session 1873-4.


Sounding Wire
Guard for catching the Wire in case of its getting out of Pulley
5 Fathoms
Taffrail
Counterpoise for Castor
SLIDE FOR RUNNING SOUNDING WHEEL IN AND OUT
AUXILIARY PULLEY FOR HAULING IN
SIDE ELEVATION.

ratus itself. By study of these drawings, with the aid of the brief explanatory notes written upon them, together with the explanations here following, as noted from the lecture, the reader may arrive at a good conception of the nature of the apparatus, and of the sounding process for which it is adapted.]

The wire is coiled on a large wheel (of very thin sheet-iron galvanised), which is made as light as possible, so that when the weight reaches the bottom the inertia of the wheel may not shoot the wire out so far as to let it coil on the bottom. The avoidance of such coiling of the wire on the bottom is the chief condition requisite to provide against the possibility of kinks; and for this reason a short piece of hemp line, about five fathoms in length, is interposed between the wire and the sounding-weight; so that, although a little of the hemp line may coil on the bottom, the wire may be quite prevented from reaching the bottom. A galvanised iron ring, of about half a pound weight, is attached to the lower end of the wire, so as to form the coupling or junction between the wire and the hemp line, and to keep the wire tight when the lead is on the bottom and the hemp line is slackened. The art of deep-sea sounding is to put such a resistance on the wheel as shall secure that the moment the weight reaches the bottom the wheel will stop. By "the moment" I mean within one second of time. Lightness of the wheel is necessary for this. The circumference of the wheel is a fathom, with a slight correction for the increased diameter from the quantity of wire on. Whatever length of wire is estimated as necessary to reach the bottom is coiled on the wheel. For a series of deep-sea soundings, in depths exceeding 1,000 fathoms, it is convenient to keep a length of 3,000 fathoms (about 43 lbs.) coiled on the wheel. When we do not get bottom with 3,000 fathoms, the process of splicing on a new length of wire ready coiled on a second wheel is done in a very short time —two minutes at most. The friction-brake which you see is simpler in construction than that shown to the Institution of Engineers in Scotland last session, and sent out a year ago to the American Navy Department. The brake on the sounding-machine now before you is a return to the simple form of brake which I

used in June, 1872, when I first made a deep-sea sounding with pianoforte wire in the Bay of Biscay, in 2,700 fathoms.

A measured resistance is applied systematically to the wheel, always more than enough to balance the weight of the wire out. The only failures in deep-sea soundings with pianoforte wire hitherto made have been owing to neglect of this essential condition. The rule I have adopted in practice is to apply resistance always exceeding by 10 lbs. the weight of the wire out. Then, the sinker being 34 lbs., we have 24 lbs. weight left for the moving force. That, I have found, is amply sufficient to give a very rapid descent—a descent so rapid that in the course of half an hour, or fifty minutes, the bottom will be reached at a depth of 2,000 or 3,000 fathoms. The person in charge watches a counter, and for every 250 fathoms (that is, every 250 turns of the wheel) he adds such weight to the brake-cord as shall add 3 lbs. to the force with which the sounding-wheel resists the egress of the wire. That makes 12 lbs. added to the brake-resistance for every 1,000 fathoms of wire run out. The weight of 1,000 fathoms of the wire in the air is $14\frac{1}{2}$ lbs. In water, therefore, the weight is about 12 lbs.; so that if the weight is added at the rate I have indicated the rule stated will be fulfilled. So it is arranged that when the 34 lbs. weight reaches the bottom, instead of there being a pull, or a moving force, of 24 lbs. on the wire tending to draw it through the water, there will suddenly come to be a resistance of 10 lbs. against its motion. A slight running on of the wheel—one turn at the most—and the motion is stopped. The instantaneous perception of the bottom, even at so great a depth as 4,000 fathoms, when this rule is followed, is very remarkable, and has been particularly noticed by Commander Belknap in reports of his soundings in the Pacific, presented to the United States Navy Department.

As to the modes of accelerating the process:—first, when there are plenty of men available, instead of handles I put on each end of the shaft of the auxiliary hauling-in pulley a smaller pulley with a sharp V-groove. An endless rope passed half round each of these V-pulleys, and kept tight by a snatch-block suitably placed in-board,

allows any number of men to haul, hand over hand, or walking along the deck, as may be found most convenient. Or, when there is a donkey-engine, it may be employed on one of the endless ropes instead of a multitude of men on the two. By multiplying the speed of men, or using a donkey-engine in that way, there is no difficulty in hauling-in the wire at the rate of about eight nautical miles an hour. Thus the last 1,000 fathoms of wire, with 34 lbs. sinker attached, may in any case be easily and safely got on board in seven or eight minutes; but a dozen men hauling together might be required for this speed. When greater lengths of wire are out, slower speeds of hauling are required for safety. With 3,000 fathoms of wire out, probably an average speed of four miles per hour (or 400 feet per minute) would not give more than from 100 to 120 lbs. whole pull on the in-coming part of the wire (or from 30 to 50 lbs. resistance of the water, added to 34 lbs. weight of sinker and 36 lbs. weight in water of the wire); and would, therefore, be a safe enough speed. Of course, if there is a heavy sea, augmenting considerably the maximum stress above the mean stress, then slower hauling must be practised. An arrangement by Professor Jenkin can very readily be applied, by which the men or engine can haul-in as fast as they please, and be unable to put more than a certain force on the wire. Thus will be realised in speed the benefit of abundance of power. The wire will come in fast when the strain is easy, and not come in at all when the ship is rising and producing such a pull on the wire as might break it if being hauled in at the moment.

The advantages of the pianoforte-wire method are very obvious. You see the simplicity of the apparatus, and the comparative inexpensiveness of it; no donkey-engine required, no three or four hundred pounds of iron cast away every time, as in the ordinary method of deep-sea soundings: and withal there is a very much surer sounding than the ordinary process can give at the same depths. The apparatus at present in use in our navy, which is better than that of any other navy in the world at this moment, except the American, is, as I know by actual experience of it, more difficult and tedious, and less sure at

500 fathoms, than sounding by the pianoforte wire at 2,000 fathoms. And lastly, there is the possibility of effecting a sounding in cases in which, as in the case of the *Challenger* in the Gulf Stream, the most matured previous process fails altogether. I think it highly desirable that the new method should be taken up by our own Admiralty. But innovation is very distasteful to sailors. I have a semi-official letter to the effect—"When you have your apparatus perfected we may be willing to try it." I may say that it seems a little strange that after my having intimated, in the month of July 1872, the perfect success of pianoforte wire for sounding in depths of 2,700 fathoms, the *Challenger* was allowed to go to sea without taking advantage of this process, and that a year and a half later I should be told—"When you have perfected your instrument we may give it a trial." The American Navy department looked upon the matter with different eyes, and certainly treated my proposal in a very different spirit. They found my apparatus full of defects. They never asked me to perfect it, but they perfected it in their own way, and obtained excellent results. I went on independently in another line, and made a considerably different apparatus from that which is now being used by the Americans; but I certainly was very much struck by the great zeal and the great ability which the American naval officers showed in taking up a thing of this description, which had merely been proved to be good, and charged themselves with improving the details and making it a workable process.

If I may be allowed two or three minutes longer, I will describe the method of making flying soundings with wire. In the first *Hooper* expedition, to lay the first section of the Western and Brazilian Company's cable from Pernambuco to Para, the Brazilian Government sent the gun-boat "*Paraense*" with us to take soundings, but the coal would not carry her the whole way, and over the remainder of it we were left to our own resources for soundings. Wire soundings had been taken over the route previously by Mr. Galloway, in a steamer chartered for the purpose by the Western and Brazilian Telegraph Company, and again in the *Paraense*, so as to give a general idea of the line to be taken for the cable; but

still it was very important that soundings should be taken during the actual laying. Accordingly, Captain Edington arranged that my sounding-wheel should be set up over the stern of the *Hooper*, and soundings were taken every two hours without stopping the ship. A 30 lbs. weight was hung by a couple of fathoms of cord from the ring at the end of the wire. Then the wheel was simply let go, with a resistance of about 6 lbs. on its circumference, the ship running at the rate of 4½ knots, relatively to the surface-water (or at 6 knots relatively to the bottom); and after, perhaps, 150 fathoms had run out—in some cases 175 fathoms—suddenly the wheel would almost stop revolving. In half a turn it was obvious that there was this sudden difference, which showed that the sinker had reached the bottom. The moment the difference was perceived, the man standing by laid hold of the rim of the wheel and stopped it. Thus we achieved flying-soundings in depths of 150 fathoms, with the ship going through the water at the rate of 4½ knots, and obtained information of the greatest possible value with reference to the depth of the water and the course to be followed by the cable. I think this is of such great importance that I never would like to go to lay a cable without an apparatus for flying soundings. The warning that this practice gives of shallow water, or of too great a depth of water, has a value which the members of the Society of Telegraph Engineers will readily appreciate. It will also, no doubt, be found useful in ordinary navigation.

There is one interesting topic to which I may refer, in conclusion, and that is the sound continually produced by the wire. All the time we are employing pianoforte wire in this way we have "sounding" in a double sense. During the whole process of sounding we are continually reminded of the original purpose of the wire by the sounds it gives out. A person of a musical ear can tell within a few pounds what pull is on the wire by the note it sounds in the length between the castor-pulley at the stern and the haul-in drum which is about five feet in-board of it.

Mr. Froude said that he had the pleasure of seeing the first specimen of sounding-wire which Sir William Thomson had made

before it was used. He forgot whether at that time Sir William Thomson had hit upon his extremely ingenious and nice mode of taking the strain off the wheel; but one could see at once that it was perfectly easy and must succeed. The idea of making a strong joining in the wire by a long succession of weak fastenings seemed a very instructive mode of getting over a serious difficulty. He saw from the beginning that the points might be put together by solder, but it was obvious to him that the stiffening of the wire by the solder must have a bad effect. He thought that it was much to be regretted that the Admiralty did not at once take up Sir William Thomson's process. They were much more disposed to take up such things than they used to be; and he was rather surprised, knowing how high Sir William Thomson's authority stood, that they should have any hesitation in taking up the process. He (Mr. Froude) wished to ask how the difficulty of corrosion by salt-water was ultimately overcome. The use of oil had been under discussion.

Sir WILLIAM THOMSON said that there were two methods of guarding against rust. The Americans used oil — submerging the wheel in oil when it was out of use. Commander Belknap having carried out the process of wire-sounding with remarkable success, he (Sir William) supposed that the Americans were satisfied with the preserving power of the oil thus used. On board the *Hooper* the deep-sea sounding-wire was preserved by caustic soda when out of use. That substance, when bought wholesale, was so inexpensive that the cost of that mode of keeping the wire from corrosion was not worth speaking of. There was, however, a good deal of trouble connected with it; but it must be remembered that that trouble would not be much regarded on board a ship appointed especially for making soundings. The preserving effect of alkali upon steel was well known to chemists. It seemed to be due to the alkali neutralizing the carbonic acid in water, for the presence of carbonic acid in water was the great cause of iron being corroded. The fact was well established that iron would remain perfectly bright in sea-water rendered alkaline by a little quick-lime. Caustic soda was a more sure material, because with it we could

make more certain that the water was really alkaline. He was told by a very excellent authority, Mr. James Young, that, whether caustic soda or quick-lime was used, all that was necessary, in order to make sure that the pickle would be a thorough preserver of the wire, was, that it should be found to be alkaline when tested with the ordinary litmus test-paper. The American experience was, that, although the caustic soda preserved the wire, it ate away the solder, and on that account they preferred to use oil.

Mr. MATTHEW GRAY: I do not know that I can add to the information which Sir William Thomson has given; but about sixteen months ago I laid a cable across the Gulf of Lyons, and, having heard what had been done by Sir William Thomson, I had an apparatus made very much like this. In going to sea with it we found no difficulty in taking soundings in 1,300 fathoms. I was, however, not so fortunate as Sir William was in getting good wire of sufficient strength to pull up the weight, and the wire was broken three or four times. At last we constructed a weight which got left at the bottom when it touched the bottom, and with that we were pretty successful. We used oil for keeping the wire from rusting. The oil was kept in a trough, and a flat india-rubber bearing rubbed against the wire to rub off the superfluous oil and keep it from flying over the deck. That seemed to answer the purpose very well. I may mention that I also had an apparatus with a view to picking up a larger portion of the bottom than is usually done. It was shaped something like a sugar-tongs, but the jaws were not fitted with sufficient accuracy, and the water washed out the material which the tongs took up. I have no doubt that in all cable-laying in the future much will be done with the apparatus which Sir William Thomson has constructed.

CAPTAIN HULL, R.N.: From the way in which Sir William Thomson has explained this process, if I were sent out to make deep-sea soundings I should apply to have my ship fitted with this apparatus. There are many things pointed out to-night which I should like to try for myself, because it is different on the table here to what it would be on the deck of a ship. I think some modification might be made in the mode of taking flying soundings by the use of an

oldfashioned implement used by sailors, which consists of a pulley connected with a little canvass bag, which in the case of the *Hooper* would indicate when the bottom was reached by bobbing up and down in the way that a float does in ordinary angling. I would repeat, if I were required to make deep-sea soundings, I should apply to be provided with Sir William Thomson's apparatus.

Mr. Matthew Gray: May I ask Sir William Thomson to be good enough to explain how it happened that the pulley was so much injured by the weight of the wire? Probably my pulley might have been somewhat stronger than his. I should be glad to know whether Sir William brought anything up from the bottom, and how he did it?

Mr. Latimer Clark: In what way is the friction strain on the wheel regulated and adjusted? I should also be glad to hear whether in the flying soundings the sinker is recovered?

Professor C. Maxwell asked whether cross-filing of the wires at a splice for increasing the grip did so much harm by weakening the wires as to counterbalance the advantage of the increased hold?

Sir W. Thomson in reply to the questions that had been put said: With reference to the question that has just been put by Professor Clerk Maxwell, I should say that cross-filing to prevent slipping would injure a wire by diminishing its strength, more than would compensate for the advantage in increasing the friction; and marine glue is found to answer so well, that I do not think any process of roughening the wire is necessary. I have been asked to explain how the resistance is applied on this apparatus. I will state in the first place that this form of brake was patented by me in 1858, and I have used it myself ever since.

Demonstrating the use of the brake, Sir W. Thomson remarked: The rate of change of pull in the cord per radian* round the wheel is equal to the amount of the pull at any point, multiplied by the coefficient of friction. The whole tangential resistance

* "Radian" is a most valuable word, introduced by Professor James Thomson to denote the angle whose arc is equal to radius. It is the hitherto nameless "unit angle" of the Cambridge and other mathematical books.

which the cord applies to the circumference of the wheel is equal to the excess of pull at one end above that at the other end of the cord. I have been asked by Mr. Latimer Clark whether I recover the sinker in flying soundings. Always; I never lose a pound of lead if I can help it. In the use of the "deep-sea lead" of ordinary navigation, six men have a heavy haul to bring up a lead in soundings of 50 or 60 fathoms, if the ship is under way; but by the wire process a cabin-boy can bring a 34 lbs. sinker with ease from a depth of 150 fathoms, the ship all the time going on her course, at from four or five knots (to which the speed may have been reduced for a couple of minutes for the sounding) up to full speed.

An important merit of wire for deep-sea sounding is the setting of the ship in motion again, which it permits almost as soon as the bottom is reached. Suppose the depth found 3,000 fathoms, by the time you have got about 500 fathoms of wire in, you steam slightly ahead; when 1,500 fathoms are in, you may steam at five or six knots without injury; and by the time you have only about 1,000 fathoms out you may steam at 10 knots; and, if the speed of the ship is equal to it, you may steam at 12 knots with 700 or 800 fathoms of line out. In fact, the time spent in deep-sea soundings will be reduced to a small fraction of what it is by the process of our own Admiralty. Mr. Siemens has asked, how quickly a sounding of 2,000 fathoms can be made. The wire, with 34 lbs. sinker, would take not more than 30 minutes to run out; but if, for a *tour de force*, you wished to do it quicker than that, I should use a much greater weight, say 150 lbs., with detaching trigger. Supposing, however, the 34 lbs. sinker to be used, with the multiplying speed on the pulleys, and twelve or fourteen men hauling on the endless rope, it might be hauled from a depth of two miles in about 15 minutes. Thus the whole process, with the recovery of the sinker, would be performed in 45 minutes. The process without recovery of the 150 lbs. sinker may be made with only about twenty minutes' detention when the object is to make a sounding with the least possible detention, and, therefore, the ship is allowed to go on her course at fair speed during the time of

hauling-in the line, with tube and specimen of bottom. A sounding of 1,000 or 1,500 fathoms, with recovery of the 34 lbs. sinker, may be executed with only the detention of stopping the ship, keeping her stopped for a quarter of an hour or twenty minutes while the lead is going down, and then going a-head full speed as soon as it has struck the bottom.

A question has been asked, with reference to flying soundings, as to the allowance to be made for the non-verticality of the wire. I have indicated that these are only approximate soundings, but they are sufficiently near for many practical purposes, and a little experience gives data for making allowances with considerable accuracy. [This was demonstrated by a diagram on the board.*] With the aid of a little experience of what the wire really does in moving through the water in flying soundings, you may obtain very close results. In the *Hooper*, I believe, the flying soundings in from 170 to 40 fathoms were ascertained within from 10 per cent. to 3 per cent. of the actual depth.

I hope my friend Mr. Froude may be induced to take up the subject of the resistance of the water against steel wire. He has apparatus at Torquay by which he measures the resistances experienced, by models of ships, which I think might also be applied to the measuring of resistances experienced by wire, and from that some valuable results might be obtained. I have found the resistance in towing, at seven or eight knots, 1,500 fathoms of pianoforte wire, with ring, short hemp line, and 30 lbs. sinker at the end, is quite manageable.

In reply to Mr. Gray, I may state that we brought up specimens of the material of the bottom by means of a tube fitted simply with a common door-hinge valve. The tube came up full of mud where the material was soft. There are a great many different plans of doing this, but we found no difficulty in getting specimens of the bottom with this tube and simple valve.

Sir WM. THOMSON having completed his explanation and resumed the Chair,

* This demonstration is given in a note on "Flying Soundings" appended to the present Report.

Mr. C. W. SIEMENS said: I may be allowed to make one or two observations upon this interesting communication which Sir W. Thomson has made to us; and I would say, like many other mechanical arrangements which have been brought before us, this is not absolutely new, and I am not surprised to hear that attempts have been made to sound by wire instead of hemp-line. But the merit of the present apparatus, as well as of any other well-devised mechanical arrangement, consists of the appliances to make the result a perfect one, and in that respect I think the apparatus described this evening commends itself without any words from me. There are many difficulties which present themselves at first sight against the use of wire for soundings, but these have been met in the most perfect and ingenious manner. First of all, to get wire of such uniform strength as to reach to a depth of three miles required very considerable attention. Nevertheless pianoforte wire offers extraordinary strengh and toughness, and is, undoubtedly, the right material; but how to join these wires in such a manner as to be reliable was a matter of great consideration, and that difficulty has been met in the most perfect manner. Then the mode of checking the motion of the drum by a single rope, although in itself involving only a Prony-brake, is a very ingenious mode of adapting a means to a particular end, and this is brought in usefully for telling in the most absolute manner when the weight strikes the bottom. As Sir W. Thomson says, attaching the weight itself to a piece of line, and adjusting the friction in such a manner as that the motion of the machine is stopped the moment the lead reaches the bottom, is another stage in the perfection of this method of sounding. There are other points of great ingenuity in the apparatus now before us. With regard to the practical value of taking deep-sea soundings by wire I have no doubt. I have myself made deep-sea soundings, and I know that in depths of 2,400 or 2,700 fathoms it occupied from four to five hours, and it was a difficult matter sometimes to keep the ship over the line. The lateral friction of the line in the water was so great that the lead did not pull, and therefore the ship had to be kept over the line. Instead of occupying five hours this apparatus completes a deep-sea sounding in

about 35 or 40 minutes, and that is a matter really of the highest importance, especially in making soundings for submarine cables, where time is a great object. Flying soundings are matters of great interest. I did not quite follow Sir W. Thomson's illustration. He shows that the lead touches the ground at a distance at least equal to the depth. I should have thought the point where it struck the bottom would be a distance from the stem of the ship not exceeding one-fourth part of the depth of the water, and the result would be that this [pointing on board] would be 10 or even 50 per cent. longer than the vertical line. In this respect I think Sir William demonstrates against himself; but if we can lay down any certain rule this apparatus is a great achievement.

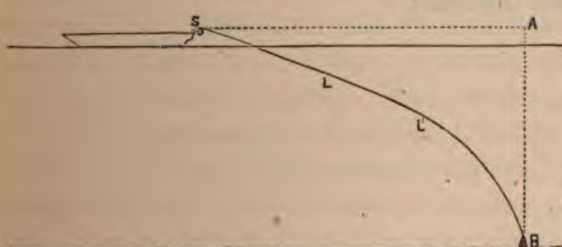
I have sent the wire-sounding apparatus out with every ship I have had lately to fit for sea; and I am quite sure the Meeting will accord a hearty vote of thanks to Sir William Thomson for his valuable communication.

Sir Wm. Thomson said, as to where the lead struck the bottom was of importance, and he gave an extreme statement against his own case. [Sir Wm. Thomson further illustrated this point by diagrams on the board, and promised to append to the report of this evening's proceedings a short demonstration, with diagram, illustrative of flying soundings.] In conclusion, he said the thanks of the Meeting were due to Mr. Siemens for having afforded him this evening the opportunity of exhibiting and explaining the apparatus, which was constructed for the cable-ship *Faraday*, and which was shortly to start on a submarine cable expedition.

APPENDIX ON FLYING SOUNDINGS.

Approximate soundings of great use, both in cable-laying and in ordinary navigation, may be obtained in depths of 200 fathoms, or less, with remarkable ease, without reducing the speed of the ship below five or six knots, even when the wire is being paid out. For this purpose let the weight fall direct from the wirewheel over the taffrail, with a brake-resistance of from five to ten pounds. The

moment of its reaching the bottom is indicated by a sudden decrease in the speed of rotation of the wheel. The moment this is observed, a man standing at the wheel grasps it with his two hands, and stops it. Not more than three or four hundred fathoms of wire having run out, the hauling-in is easy. In following this process I have generally found it convenient to arm the lead with a proper mixture of tallow and wax, in the usual manner, to bring up specimens from the bottom. The actual depth is, of course, less than the length of wire run out. The difference, to be subtracted from the length of wire out to find the true depth, may be generally estimated with considerable accuracy after some experience. The estimation of it is assisted by considering that the true depth is always, as we see from the annexed diagram, greater than $l - a$ and less than $\sqrt{l^2 - a^2}$, where l denotes the length of wire out,



$$l = SLL'B$$
$$a = SA$$
$$b = AB$$
$$l > \sqrt{(a^2 + b^2)}$$
$$l < (a + b)$$
$$\therefore b < \sqrt{(l^2 - a^2)}$$
$$b > (l - a)$$

Flying Soundings.

and a the space travelled by the ship, diminished by the space travelled horizontally by the sinker during the time of its going to the bottom.

The contrast between the ease with which the wire and sinker are got on board from a depth of 200 fathoms by a single man, or by two men, in this process, and the labour of hauling-in the ordinary deep-sea lead and line by four or five men, when soundings are taken in the ordinary way from a ship going through the water at four or five knots in depths of from 30 to 60 fathoms, is remarkable. Professor Jenkin and I found this process of great value on board the *Hooper* during the laying of the Western and Brazilian Telegraph Company's cables between Para, Pernam-

buco, Bahia, and Rio Janeiro. I am now having constructed, for the purposes of navigation, a small wire wheel of 12 inches diameter, to have 400 fathoms of pianoforte wire coiled on it, for flying soundings in depths of from 5 to 200 fathoms, without any reduction of the speed of the ship, or, at all events, without reducing it below five or six knots.—W. T.

The following Candidates were balloted for and declared duly elected:—

John R. Brittle . . .	3, Great George Street.
Oliver Heaviside . . .	Newcastle-on-Tyne.
G. Richardson . . .	Calcutta.

The Meeting then adjourned.

The Twenty-seventh Ordinary Meeting was held on Wednesday, the 13th May, 1874, Mr. LATIMER CLARK, Vice-President, in the Chair.

Adjourned Discussion on Mr. LANGDON'S Paper "ON THE DECAY AND PRESERVATION OF TELEGRAPH POLES."

Mr. LANGDON: I am anxious to have the opportunity of saying a few words supplemental to the paper which I have had the honour of submitting to the Society, in order to lay before you some additional specimens of telegraph poles, showing their condition after having been in use for several years. Through the kindness of Mr. Culley I am able to place before you specimens of the poles referred to in the paper as partially creosoted—that is, creosoted at the butt end only. They are fair specimens of the process, after some eight hours' steeping. By the kindness of Mr. Goldstone, the Telegraph Superintendent of the London and South Western Railway, I am also able to produce one of the poles spoken of as having been in existence some twenty-five or twenty-six years on the Fareham and Portsmouth branch of that Railway. To all appearance it is now in as sound a condition as when first planted. The portion which I submit to your notice is cut so as to possess about two feet of that portion which when the pole was in position was beneath the soil, and two feet above the same. This pole was removed last month, and has been brought here as a specimen of the durability of creosoted timber used for this purpose. Lieutenant Ramsey has also furnished me with a specimen of a boucherised pole planted three years ago, which broke off short at the ground line, thus supporting one of the theories advanced in the paper to the effect that all these preparations tend to destroy in a great measure the flexibility of timber.

In the course of the short discussion that followed the reading of the paper Mr. Latimer Clark asked whether any one had

ever heard of a case of decay in creosoted timber. That induced me to write to several gentlemen connected with the profession to ask them whether such an instance had ever come under their notice; amongst others, Mr. Newman of the London and North Western Railway; Mr. Warwick of the Midland, who has thirteen years' experience of creosoted poles; Mr. Cripps of the Brighton Railway, six or seven years; Mr. Haynes of the Bristol and Exeter Railway, eleven years; and Mr. Sach of the Great Eastern Railway, who has fifteen years' experience. These all agree that creosote is the best preservative they have had under their observation, and none of them have any knowledge of a decayed creosoted pole. Mr. Webber, of the South Devon and Cornwall Railways, however, has forwarded some specimens of what he calls creosoted timber, which is in a state of great decay (specimens shown). Perhaps it will be as well to read that portion of Mr. Webber's letter which has reference to that specimen :—

" Dear Sir,—

" In reply to your letter of the 27th inst. I have not had sufficient experience to give an opinion on the durability of creosoted telegraph poles. The prepared poles on the South Devon and Cornwall Railways are boucherised and kyanized. We had no creosoted telegraph poles until those recently introduced by the Post Office.

" Yesterday I sent you a piece of creosoted timber I took from a bridge under repair, about one mile east of Totnes. It had been in use about seven or eight years. It was in gravelly soil, and water could drain through the bridge under the Railway. There were other pieces in the same condition, the timber being of small dimensions. The creosote must have penetrated well into the wood, you can see, by its colour."

That led to an inquiry as to the position occupied by these pieces of wood, and this is the reply I received :—

" I am unable to say how far the decay extended in the particular piece of wood I sent you, as it was lying with other pieces taken from the bridge, some of which were decayed for two or three feet."

I am indebted to Mr. W. W. Bateman for calling my attention to an error which I find has crept into the paper. On page 13 the material employed in the Burnetizing process is stated to be sulphate of zinc: it should be chloride of zinc. Mr. Saunders calls my attention to a statement in the paper with reference to iron-socketed poles, and writes:—

"At page 12, I observe that a statement has been made to the effect that the poles which had been placed in screw iron sockets between Yatton and Worle Stations had stood for twenty years without renewal. This line was, as you are aware, for nearly the whole period stated under my charge as engineer and superintendent, and during that time a large proportion of these poles certainly were renewed, and I much question if any of the original poles were standing when Mr. Haynes made his report. Doubtless poles placed in these sockets stand exceedingly well, at the same time it is better that the actual facts of the case should be made known.

"For many years past I have strongly advocated creosote in preference to any other preservative; as, even when only indifferently done, it certainly adds considerably to the durability of the poles. On the Taff Vale Railway the system in operation for creosoting is, without question, the most perfect in existence: The poles (and other timber) used on the railway are seasoned in the ordinary way—that is, by stacking, and then placed on fireproof trollies. The timber is then put into a drying-house—that is, a building which can be heated to a very high temperature—and, as soon as it is considered the whole of the moisture has evaporated, the poles, when still hot, and on the same trollies, are wheeled directly into the creosoting tank, out of which the air is immediately exhausted; and, when this has been done, the injection tap is opened and the creosote suffered to pass therein. Pressure is subsequently applied, and the result, as you may readily imagine, is most satisfactory—as I have found, even with the best description of timber, traces of the oil can readily be detected throughout every portion of the pole."

I hereon thought it desirable to address Mr. Haynes, and he replies :—

"I have to thank you for your letter of the 14th inst., and you will be pleased to hear that I should not have ventured to offer any information respecting the 'poles in iron sockets' had I not been able to confirm it.

"There is a workman in the service of this Company who altered the *arms* on the poles twelve years since, and the poles had then been standing a number of years. The slots in the poles from where the arms were removed can now be seen. The alterations were made on account of the old "No. 3" insulators being abandoned, and the "Invert" form of insulator used in their stead. It was from this workman that I also received the information with respect to no renewals having been necessary, and as he is a honest and truthful man I can rely on his statement.

"We have found it necessary to renew some poles on the section referred to since my communication of last year. The repairs for the present year have just been completed."

Mr. Haynes has sent me a specimen of one of the poles removed. I did not think it worth while when I read my paper, as so many specimens were submitted to you, to trouble you with any more, but this [*pointing*] is a portion of one of the original poles planted, which, you see, is decayed at the portion where the junction with the socket is made. Mr. Woods, who is now connected with the postal service (but who was formerly engaged on the line on which these poles were planted), writes :—

"I think there is no doubt but what the iron sockets between Yatton and Weston Junction on the B. and E. Railway were fixed about twenty years ago. During my service with the B. and E. Railway only about two of the poles, to the best of my knowledge, were removed.

"I should think by appearance the majority of the poles first fitted to the iron sockets have been in use until within a few months."

This confirms the statement of Mr. Haynes, that many of the

original poles are still standing; and it is fair to conclude that Mr. Saunders was mistaken in his statement.

I should add, there is little doubt of the specimen just referred to being one of the original poles first planted. It bears marks which go far to identify it as such; as, for instance, the fastening of the earth-wires with hooks instead of staples. It is slotted also for the old No. 3 insulator, superseded, as Mr. Haynes remarks, some years since.

Mr. W. H. Preece has placed in my hands a letter received from Mr. W. H. Hyett, a gentleman whose name has been referred to in the paper. The letter will be placed in the hands of the Secretary, and, when published, will, I have no doubt, be read with very great pleasure.

Mr. Sach: I have tried a great many experiments on my district under the old Electric Telegraph Company during a period of twenty to twenty-five years, and, to sum up the result shortly, I am perfectly satisfied that creosote is very much superior to anything within my experience.

My continental line poles are creosoted; they have been in the ground now about fifteen years, and show no sign of decay up to the present. Only one has been removed during the whole of that time; they remain apparently as sound as when they were placed there. I had a few creosoted poles (about a dozen) mixed in with a large delivery of unprepared about nineteen or twenty years ago; they also are sound now, although all the others in the same locality have disappeared years back.

I think the principle of preserving our poles is better understood now than it was formerly; it is generally admitted we must season them—get rid of the sap,—something must take its place to act as a preservative. Whatever this be, it should contain properties to reject water—prevent in fact any absorption of moisture. Creosote, being of an oily nature, does this; but it is not sufficient to put the oily matter into the pole—we must endeavour to keep it there. I have always found a good coat of tar answer the purpose. I have heard it stated as one objection to creosote that it sinks down and out at the bottom; but, if we manage to prevent this for about a foot or

eighteen inches from the ground-line, there is little fear of losing it lower down.

In tarring above ground, it is advisable to wait a few months after the poles have been set; the surface is in better condition to take the tar, which will prove of great service, and prevent exudation in warm weather.

Mr. GRAVES: The question of the duration of timber is, perhaps, commercially considered, the most interesting topic that could be started in connection with overhead telegraphs. Upon the life of the poles used the financial success of any extensive system of lines must very greatly depend. Renewal at short intervals means the frequent repetition of a very large part of the original construction expenditure.

At the present date the telegraph engineer is practically limited in his choice to three classes of materials—larch, boucherised, or creosoted poles. Square Memel is too costly for use, except to a small extent under special conditions. Norwegian or Swedish timber decays too rapidly to be employed without preservation.

Larch varies greatly in quality (as pointed out by Mr. Langdon) in accordance with the varying conditions of its growth. Of late years it has increased greatly in price, and in some quarters is not to be obtained without an additional heavy charge for carriage. Its employment in certain cases will no doubt continue under circumstances arising from considerations of locality and cost. But I believe it may be assumed that, other things being equal, preserved timber will be generally preferred, owing to its proved advantages in point of average duration.

Boucherised poles, in this country at least, can hardly be said to have proved themselves a success. Theoretically the process is attractive, its application is simple—no tedious preparatory seasoning is called for, and the telegraph posts can often be procured and made ready for use on the ground where they are required for erection. On the other hand, suitable raw material is not always easy to procure. Scotch fir having a very open grain is the wood selected for preservation by the injecting process; but Scotch fir is not readily found in quantities sufficiently straight and tall to admit

of large numbers of poles being cut in any one plantation in proper condition.

In 1862 and 1863 I superintended the boucherising of over 6,000 poles in Lincolnshire. The timber was too old. In some instances a month elapsed before the copper liquid applied to the butt appeared on the top of the poles. Externally sound, there is no doubt that incipient decay existed in many of the trees supplied to us, and that the sap channels, so to speak, were partially closed. The result was in accordance with the facts. Some of the boucherised poles turned out well, and are good to this day, others were greatly decayed in three or four years. To boucherise successfully, the very worst natural timber (I mean as regards its chance of lasting unprepared) is wanted—it should be rapidly grown and very open-grained. In some instances suitable wood was obtained—in many it was unsuitable. This I believe to be the main explanation of the varying experiences I have heard of.

At Hovingham, in Yorkshire, in 1861-1862, I boucherised about 2,000 poles. On the whole the work was more satisfactorily done than in the Lincolnshire case. The liquid passed through more freely, and I understand that the poles have on the average lasted better. Many of them erected on the North Eastern Railway in 1862 are reported sound now.

I concur with Mr. Langdon in preferring creosoted timber to any other kind now generally available. It combines comparative economy in first cost with the best chances of satisfactory duration.

It cannot, however, be safely assumed that all poles are sure of long life simply because they are creosoted. The instances of the London and South Western experience quoted by Mr. Langdon are highly favourable—others can be adduced that at first sight seem less so.

In 1866 a large number of half-squared Norway poles, *i. e.* poles with two sides fully square and the others meeting at a rounded corner, were creosoted in Hull, and erected on the Hull and Milford branch of the North Eastern Railway. Many of these are said to be now far gone, although from appearances the creosote would seem to have penetrated freely.

In 1859 or 1860 round Norway poles (creosoted) were erected on the Nottingham and Grantham branch of the Great Northern Railway; some of these are reported to be apparently still sound, indeed many of them, but others have needed to be replaced in consequence of decay.

From Railway Engineers I have obtained most contradictory opinions as to the practical result of creosoting, yet these opinions were in all cases professedly (and I believe really) founded upon the teachings of experience.

As with boucherising so with creosoting, satisfactory results can only be assured by careful observation of essential precedent conditions.

It is of the first importance to provide for the absolute dryness of the timber placed in the creosoting tank—I do not at all believe in the theory of complete exhaustion of the sap by the exhaustion of the air. Experience contradicts it. The wood should be free from both natural sap and absorbed moisture, and, simple as these conditions seem, they are very hard to secure—they mean, in fact, a lengthy process of seasoning under covered sheds, through which the wind can blow freely. So far from this being done in the cases of creosoting to which I have alluded, the poles were, I know in some instances, palpably saturated with moisture when placed in the tank. The exhaustion process was relied on. The result has been experienced in decay.

Storage for a long period implies the provision of heavy standing stocks of timber and serious cost for rent and other charges. Hence financial reasons often compel the use of poles that have not undergone a sufficient preliminary preparation.

When placed in the tank it is important to determine the quantity of creosote to be forced into the wood—I have known it to range from 4lbs. per cubic foot to 12lbs. or more, but it would seem that 8lbs. or thereabouts honestly absorbed is sufficient. Careful inspection of the operation is all important, and a certain number of representative poles taken at random should be weighed and measured by the inspector out of each charge placed in the tank. When taken out they should be again weighed, and from them the

average result can be fairly gathered. When the creosoting is done by contractors such a check is an absolute essential—I believe that in the early days of telegraph creosoting much oil was paid for of which the timber never reaped the benefit.

The system of partial creosoting, *i. e.* of coating the outside of the lower portion of poles, can, I think, only be regarded as the application of what I may call a rarefied coat of tar, and I presume that except for experiment or under very exceptional circumstances its use is not likely to be resorted to on any large scale. At any rate, be its advantages what they may, they cannot possibly equal those of full-creosoting, even allowing for the economy of first cost. The upper portion of the timber remains wholly undefended by the preparation and dependent solely upon painting or tarring. This implies liability to percolation (through the fibres of the wood) of external moisture—a liability which would be serious if the painting was not frequently renewed; while, on the other hand, if the external coating be complete the pole unless very thoroughly seasoned and dry in the first instance is placed under conditions exactly calculated to favour the rapid development of dry-rot.

I trust that one point of much importance that is raised by Mr. Langdon may meet with full ventilation—as to the necessity of periodically tarring creosoted poles. There can be no doubt of the apparent advantage of such a course, but it has several drawbacks. The main one is its cost. On many roads the local prejudice against wholly black poles raises a serious obstacle. The exudation of the creosote oil forms a tarry cake round the bottom of the poles. If the poles really must be re-tarred for safety, would it be necessary to open out the ground at the foot and disturb the leakage formation? I can pretend to no sufficient experience that would justify me in expressing a definite opinion on these points, but I trust that this branch of the question may not escape notice.

Creosoted poles should, in my belief, be allowed to remain some time in a lateral position before being erected. When this is the case the visible leakage of the oil is much less than when erection

takes place immediately after removal from the tank, and it would seem that a larger portion of the oil is permanently absorbed into the tissues of the woody fibres.

Mr. Arthur R. Granville: In addition to the processes of creosoting, boucherising, burnetising, and kyanising, there are various other methods of preserving timber; and a short statement of what Monsieur Boucherie, the son of the inventor of the boucherising system, thinks upon the subject, may not prove unacceptable to the members of this Society. He said, in the early part of the present year, before the French Academy, and in support of his father's system, that it is an error to suppose that the sulphate of copper is very soluble, or that when exposed to the rain its preservative power disappears after a certain time; this anti-septic substance, he declared, fixes itself in the elements of the wood, and could not be dissolved by washing. Cases of failure in the preservative effects of the boucherising system he attributes to a disease in the wood, the diseased tissue seeming to resist the sulphate. The fact that the French boucherise *all* their poles must speak somewhat in favour of this process. In that country, however, there seems to be a desire felt to have a cheaper system of preserving timber, and many have advocated the tannate of protoxide of iron. M. Boucherie objects to its possessing the preservative properties attributed to it. He admits that it destroys termites and worms, but does not believe it preserves ligneous tissue, for he declares that a minimum salt of iron introduced into the vascular portion of wood is rapidly converted into a maximum salt, a phenomenon accompanied invariably by the disorganization of the wood itself. Experiments were made in dyeing balls black by injecting them with sulphate of protoxide of iron, of tannin, and logwood; but it was found that the wood was only preserved from decay when dry. Injected, however, with sulphate of copper, they were perfectly preserved after twenty-five years. The following is another recipe, but of what value I cannot say, although many speak very highly of it:—Boil linseed oil, and mix it with charcoal dust until it has the consistency of an ordinary paint. Farmers who have used it declare that posts treated with a single coat of this mixture will

never rot. Regarding creosote, I do not notice that Mr. Langdon has mentioned it to be a non-conductor of electricity.

Mr. C. E. Spagnoletti said: The subject of Mr. Langdon's paper is one of great interest to those who are engaged in and have the charge of telegraph lines, because the poles form the largest item of expenditure in maintenance; therefore whatever can be done to preserve the timber and prolong the life of the poles is of great importance. I have brought with me a few specimens of timber which I think will be interesting to the meeting, and the experience I have had for some years is this. With regard to prepared timber, my own belief is that as much depends upon the quality of the timber itself as in the mode of preparing it. If unprepared larch is used, it should be good hard mountain-grown timber, felled at the proper season, and well seasoned before being planted; I have found larch poles last from seven to ten years. Charring and tarring is a doubtful question—some approve of it, others think it is not of much advantage, but I think it destroys vegetation. Boucherising does not appear to add much to the life of the timber, and we have here a specimen of a pole boucherised and planted in 1866, and you will find it is entirely gone; it was removed in 1872. These poles were boucherised on the estate on which they were grown, but as they grew rapidly in a soft moist soil they contained a good deal of sapwood, and they have not more than forty or fifty rings; consequently, though they were thicker than many other poles on the line of a much longer growth, the majority of them have gone bad, some even down to the bottom of the pole. I think one cause of the decay is, the pores of the wood are so coarse and open that the attraction of moisture causes a dilution of the preservative properties of the copper salt, and by gravitation it may work out of the wood. It is also to be remarked that boucherising is injurious to the stays, earth-wires, arm earth-wires and bolts, as the sulphate of copper so quickly attacks not only the galvanizing of them, but the iron itself, and the earth-wires from the arms, if put through the bolt-hole, are soon eaten away; and I believe it is the custom of the Post-office engineers to paint a portion of the pole where these wires come in

contact with it, to keep hte deleterious effects off the galvanised iron wire. I think some of the poles may have decayed from being bad or dead timber, and from the process having been improperly applied. With respect to kyanised timber, I may state that poles put up in 1865 are still good, with the exception of a few which were put in chalk. Some were removed in 1871 and used again; but poles thus prepared were decayed wherever they came in contact with chalk, which is equally bad for ironwork as boucherising. I have here a specimen of kyanized timber placed on the Great Western Railway in 1840, and my attention was drawn to the fact that the sapwood of this piece of baulk timber is perfect and good, as you will here see. I think with regard to kyanising, though it is an excellent preservative of wood, yet with regard to our light wires and light iron-work in contact with the pole, the effect upon them from kyanising is as deleterious as that from boucherising. I may say with regard to kyanising that 2 lbs. of corrosive sublimate at 3*s*. per lb. will be sufficient for 50 cubic feet of timber. The time for preparing should be one day per inch and one day over—thus, two days for 1 inch, three for 2 inches, and so on. One advantage of this preservative is that the timber may be cut down and cast into the tanks at once, the sooner the better, and no preparation or seasoning is required or necessary.

Textile substances so treated are, according to Faraday, almost indestructible, and some pieces of calico and canvass he prepared were found to last a very long time, while unprepared samples of the same materials rotted in a few months. Creosoting timber appears to be the best preservative we have, and the most lasting for timber used for telegraphic purposes if properly done, and if the timber before being submitted to the process is well seasoned. The first poles put on the Basingstoke Branch * in 1859 are still good, after a period of eighteen years. It has, however, this disadvantage; it makes timber exceedingly brittle. It is also an awkward preservative to use in case of poles on public roads and railway platforms, because the heat of the sun draws the creosote

* Great Western Railway.

out of the wood, and it is unpleasant if people come in contact with the poles. I erected some creosoted poles three years ago, and find the banks of the railway are saturated with creosote for a yard round the bottom of the poles in some instances. This process is particularly good for poles to be planted upright, as the heat of the summer sun every year causes the creosote to liquify, and by gravitation the portion of the pole most requiring the preservative (the bottom) is for some time continually receiving an annual recreosoting by percolation.

The CHAIRMAN: Was the vegetation dead for a yard round the pole?

Mr. SPAGNOLETTI: Yes; creosote is not destructive to the ironwork about the pole. I have found that the heat in summer was sufficient to draw the creosote out of the poles, and that it has run on to the arm, and when the earth-wire on the arm has been too near to the pole that it has been covered with creosote and thus insulated, by reason of which they are rendered useless and their object destroyed. With regard to iron sockets there have been a great many used on the Great Western Railway. They are very similar to those shown on Mr. Langdon's diagram, and poles placed in the sockets have remained twenty years, and were good when they were taken down. The difficulty is, if sockets are used, if you want to increase the capacity of your poles you must increase the size of your socket, which is expensive, otherwise the thing would look exceedingly unsightly, and cutting the pole to fit the socket would weaken the foundation of your pole. Mr. Preece has put on an iron shoe at the bottom of the socket; but I think this to a pole unprepared or imperfectly prepared would tend to hasten its destruction. By confining the natural elements of decay the moisture from the surrounding earth is not kept from the timber, and the alternate action of wet and dry would, near the surface, continually be going on; but, if the timber is well prepared and the shoe be continued as a case up the pole to rather above the ground line, this would serve to keep the preservative in and the moisture out, and would no doubt make a lasting pole. But for the trouble of moving, poles planted in concrete would no doubt

last a long time. I have a letter from one of my inspectors who has had a long experience in the maintenance of telegraphs, which is interesting, and with your permission I will read it. It is on the question of the longevity of timber.

Decay and Preservation of Timber used for Telegraph Purposes.

"Dear Sir,

"In my experience of timber I find that creosote is the best preservative of poles; I have never seen a decayed creosoted pole, and herewith send you specimens of timber that has been in use on the Worcester and Hereford line over twelve years. The pieces marked 1, 2, and 3 are sections cut from the top, ground-line, and bottom of the same pole, by which you will see that the wood is as good as when first put up. The section marked 4 is cut from the top of another pole just under the arm mortice at your request, to see if there was anything like dry rot at that point; you will see by it that the wood is perfectly sound, and since seeing you on Thursday I have examined a number of similar poles between Henwick and Bransford Road now standing, and find them perfectly sound and in as good condition as those you examined lying at Bransford Road Station. I am however informed by Lineman Worth that some time ago he took out two decayed poles of the same class from off the Malvern line. From his description of the poles I should think it was dead stuff before it was creosoted: he described the wood as crumbling to dust when pressed. The soil over the Worcester and Malvern line is clay and gravel, sometimes mixed sometimes separate. I have found all classes of timber whether prepared or not last longer in clay than any other soil.

"I also inclose you a piece of one of the cedar poles you were speaking of; the poles were sent from America to England some years ago as an experiment, and purchased by the late East India Company, unshipped at Gloucester, taken to Didcot, and stacked there. On proceeding to use them I tested one for strength, when I found it rotten right through the centre; and most of them were so soft that I probed a bar four feet into them from the

butt easily. Some I had split, and found the whole pole hollow from bottom to top, something like a piece of elder with the pith out. You will see the grain of the stuff is very close, and I have no doubt would be very durable if cut at a proper age; they were very light indeed, even the sound ones, and those decayed had evidently died long before they were cut down. I am of opinion that let poles be ever so well seasoned that moisture will arise from the earth through the pores of the wood and find its way to the air at the ground-line, where the decay commences, and it appears to be only a question of time with all classes of timber, whether specially prepared or otherwise.

"I was speaking to a master painter the other day on this subject, and he says, that if a piece of green or wet wood be painted on three sides that decay commences at once on the fourth or exposed side; if completely covered it commences to decay inside; if the paint be chipped off and a portion of the timber left exposed the decay will fly to that part at once; and, although he is a painter and a man of great experience, he says we have still to find a good preservative for timber used for outside purposes.

"Should there be any thing worth notice in the above remarks I shall be pleased, and remain,

"Yours truly,

"THOMAS MILLER.

"C. E. Spagnoletti, Esq."

I have also a communication from the same person on the subject of the durability of cedar poles, which is as follows:—

Cedar Poles.

"SIR,—The pole from which the piece of cedar which I sent you was cut had been up about seven years, and I believe there are now five or six of the same class of pole standing between Didcot Station and the junction box, bearing the railway wires, that have been there over ten years. The four pieces of creosoted pole sent you were from two poles; those marked 1, 2, 3, were from one pole, that marked 4 was from a different pole, cut from just under

the arm mortice. I have just turned up an old pole from off the Severn Valley Line; those poles were experimented upon by being bored *from* the bottom up *to the ground line*, another hole then bored into it transversly, the bottom was then plugged up, and the hole filled up with creosote and plugged up at the ground-line. I split the bottom and found that none of the creosote had entered the wood; and thinking, Sir, you would like to see a portion of the wood, I have sent a section up to you.

"Yours truly,

"THO. MILLER.

"C. E. Spagnoletti, Esq."

PROFESSOR ABEL: With regard to the question of these metallic salts, I think Mr. Spagnoletti laid stress upon the want of perfect action of copper salts, owing to the washing out of the salt. I doubt whether that is the case, and it would be interesting to ascertain. I believe a small quantity of the copper salt is converted into insoluble compound; the action of the metallic compounds is to combine with certain albuminous substances in the wood, by which they are converted into insoluble substances, and it is by the chemical alteration which these undergo that the preservative effect is brought to bear. I do not think the rotting of the wood in the first instance can be ascribed so much to the washing out of the salt, as to access of air to the wood through the more or less porous soils in which the poles are planted. If the pole is fixed in a stiff clay soil, or in hard rock, I think it is probable the preservative action is greater than if it were buried in porous soil or loose earth. I had opportunities, about six years ago, of making experiments upon the boucherising, kyanising, and other processes for the preservation of wood. Generally the results were favourable to boucherising. By the application of solution of copper salt under pressure the wood was impregnated, and after it had been buried some years it was found to have withstood decay better than other samples prepared with other metallic salts. In the year 1841 M. Boucherie himself experimented upon different metallic salts—mercury, zinc, and iron—and he came to the conclusion,

after a number of years, that a copper salt had the best preservative action. I believe opinions are fairly divided between copper salts and mercury salts, and between the boucherising and kyanising process, which undoubtedly are both good preservative processes. I gather that Mr. Langdon comes rather to the conclusion that, of the various processes classed under the head of preservation by metallic salts, that which most counteracts effects of the moisture on the wood most adds to the life of the timber. No doubt that is in a great measure true; that is, if a process is applied by which the pores of the wood are filled up by an antiseptic material, you have an action which, by the exclusion of air, prevents decay independently of the antiseptic action, and that is one reason why the creosoting process is more efficient than treatment with metallic salts. There is the twofold action, one that of filling up the pores, and the second the antiseptic action of the creosote. This brings me to another point. In former days creosote was regarded as the most valuable antiseptic agent, and the creosoting process was, as a rule, very efficient, because in it you had to the full extent the antiseptic action of the material combined with the filling-up action of the oil. Now within the last few years the substances which are the real antiseptic agents in creosote (carbolic acid, &c.) have become extremely valuable, and in ordinary commercial creosote they no longer exist in the abundance they used to do. In fact, even as far back as eight or ten years ago, when I examined a number of samples of creosote for a railway company, I found a very great difference in the proportion of carbolic acid in the samples of creosote that were offered for preserving purposes, consequently there would be great difference in the efficiency of those various creosotes. This variation exists to a greater extent now, therefore one can hardly draw a conclusion from past experience what the creosote of the present and future will be in reference to its preservative action on wood.

Mr. Rolls said: I did not unfortunately hear Mr. Langdon read his valuable paper, but I have derived much pleasure from the perusal of a copy, and I am sure we must all agree that we are

deeply his debtors for the mine of valuable information contained in it.

I think, with regard to foreign-grown timber, that we should be very careful indeed before pronouncing for or against any particular species; they one and all so vary with different conditions, of climate, soil, and elevation, that it is all but impossible for any but the connoisseurs of the timber trade to avoid often giving the place of honour to an undeserving species and slighting a worthier kind; and I notice in the paper we are discussing that the pitch-pine falls under condemnation on the ground that after a time the resin, turpentine, &c. which form its preservative, ooze out and leave only a mass of dry short woody tissue; but this can only be the case with some specimens of the genus; Oregon pitch-pine being largely used for the principal spars of vessels, demanding of course great strength and resistance to decay in all climates. I quite agree with Mr. Langdon's verdict in favour of larch. Although a young man I am an old telegraphist, and all things considered I think larch is next to creosoting; it will stand well for 10 to 15 years, and then have only a surrounding of decaying sapwood; that knocked off until the sound wood beneath is reached and that tarred, or charred and tarred, and the pole will continue to last longer than I can tell. But I am free to confess that this is larch *par excellence*, and hardly to be obtained at the ordinary price of telegraph poles, and is to be found only at certain elevations and growing upon the poorest soil. It is I know usual to insist upon natural butts. But this may not always be wise, larch even of the best sort when growing being frequently found to be foxy or decayed inside the butt, and that without future detriment to the remainder of the timber.

With respect to charring and tarring these and other plain poles I have always held the opinion that, provided the spar be tolerably seasoned, the process is of great use when properly carried out. But inattention to the theory involved frequently leads to inefficient practice. If we take a pole and gently roast, or I might say toast it at a considerable distance from the fire, the first effect will b

similar to kiln-drying, or quick seasoning; the next will be to render insoluble the albuminous matter of the wood, and thus prevent it from becoming the active element it otherwise would in the future decay of the pole. So we attain one of the elements of kyanising.

If we now bring our pole somewhat nearer the fire, so as to reduce a moderate portion of the outside wood to charcoal, not ashes, we shall surround it with a powerful antiseptic carbon; coat this with coal-tar, and we add a second and still more powerful antiseptic in the shape of its creosote, so called, and the eliminated pitch forms a waterproof envelope. As to boucherising, I quite coincide with Mr. Spagnoletti and others, that the process is of little service and gives very varying results. I have had to renew many boucherised poles that could claim only an existence of six or seven years, and in one instance a mere infant of less than two. All these were of the ordinary yellow colour. But upon the same line were a number of copper injected poles of the same kind of wood (Scotch pine), of a blackish colour, and these, although several years older than their yellow brethren, were, with one or two exceptions, perfectly sound. Reflecting upon the matter, it occurred to me that possibly the sulphate of copper used might have been impregnated with iron salt, and that the latter, entering into combination with tannic, and perhaps gallic, acids existing in the wood, had given rise to the black colour which was found in the interior. I should not expect the tannates or gallates formed in fir or pine to be in sufficient quantities to affect their preservation. Still, the idea naturally presented itself, does an admixture of iron-salt with the copper assist to prolong the life of a pole?

Of kyanising I have little or no experience. I once set 20 or 30 poles prepared after Mr. Kyan's plan, but they were shortly after removed in the course of alterations. The principle appears to be correct—the perchloride of mercury should render insoluble the albumen of the wood, and the resulting subchloride defend it from the attacks of insects and fungi. Yet, in practice, this does not appear to be always borne out, for

one of the engineers of the Croydon Atmospheric Railway informs me, that when that line was in construction Baltic timber of the best sort was selected for the sleepers, and these were steeped in wooden tanks containing the solution of mercury. The steeping completed, the sleepers were stacked and the tanks were broken up; their bottoms were found to be rotten, and the bottom sleepers of each stack were also, when got at, found to be in the same condition. However, with such a powerful ally as creosote, I think we may well dispense with the poisonous compound of mercury, attacking as it does the human body, externally and internally.

Creosoting, properly carried out, answers every purpose perfectly well, and at the present moment it is impossible to say how far it may prolong the life of a pole. I believe the Egyptian mummy-cloths are supposed to have been steeped in a liquid containing this essential principle, and we all know how wonderful is their preservation. With the same luck we should have the poles which Mr. Langdon mentions on the Portsmouth Line and others in excellent condition somewhere about the year 6,000; rather longer than we are likely to be interested in the matter. In creosote, then, we possess a preservative fully meeting our requirements, and it only remains to consider the method of applying it.

All who have experience of creosoted poles must have noticed that some, under the influence of a single summer, lose most of their creosote, and become white, dry, and brittle. Some are reduced to the same condition in a longer time; while others, and these always from the same batch of creosoting, retain their creosote, are black and shiny on the exterior, as if pitched, and become harder and harder with age. I believe these different results are due to the description of creosote used in each case; and that, as a rule, in manufacturing the article, the distillation is carried on too slowly, and the resulting liquid, being necessarily very volatile, is under the influence of sun-heat more or less evaporated and drained from the pole. But, if the distillation be urged so as to drive over into the receiver a certain amount of tarry matter, we shall then have the creosote with which the hard black poles have been prepared.

When we fail to obtain this creosote, and I know of no practical reason why we should, doubtless the next best thing is, as Mr. Langdon suggests, to coat the outside of the pole with coal-tar. This, however, could not be so efficient, as it has only a surface effect. There is another point which may perhaps deserve attention. Talking, some time ago, to the manager of some creosoting works, he said that poles exposed some time to the sun will no longer take the creosote at any pressure they can apply. If this be so, it is of course of the utmost importance that poles should be seasoned under shade. One word more and I will no longer trespass on your time. With respect to the Chairman's question about creosoted poles I may answer in the affirmative. On the West Moors section of the South Western Railway I had to renew several poles creosoted to the depth of one and two inches, which were in a very bad state. These were all of Norway spruce fir.

Mr. Langdon: I had hoped the discussion upon the paper which I have had the honour to submit to the Society would have been carried a little beyond its present limits. I had hoped that more of those railway telegraph engineers who have had so large an experience of timber applied to telegraph purposes would have favoured the meeting with some observations. No doubt such would have added largely to the interest and value of the paper. The subject is one of unquestionable importance to all railway men as well as to the Government. In the paper I have stated that roughly it may be estimated at something like a saving of £12,000 a-year if we can even effect an extension of the life of timber to the extent of 3½ years: if we can augment this to another 3½ years we shall of course double that amount, so that the question becomes one, though not of vital importance to telegraphy itself, yet of great importance to its commercial success.

On the occasion of the last meeting Mr. Bell made some inquiries with regard to some poles on the Basingstoke and Salisbury branch of the South Western Railway, planted in 1855. I am unable to state how those poles were renewed, but I believe there were partial renewals up to 1865.

Mr. Graves has instanced some cases of decay in creosoted

timber on the Great Northern and North Eastern lines of Railway. I can only say in reply to this that no such instance has come under my own notice, nor have I been able to learn from any of the old experienced hands employed on the London and South Western line of Railway, almost from its first association with telegraphy, of any such coming under their observation. At the same time I think it quite possible, where poles saturated with wet, or with the sap still in them, are creosoted, decay will ensue, but in such cases I should look for it in the interior of the pole. If it should arise in that part where the wood has taken the creosote I should be inclined to attribute it to decay in the timber at the time it was subjected to the process. A very important point also is the purity of the oil itself. In common with all who have listened to Professor Abel's remarks I feel indebted to him for directing attention to so necessary a consideration. To all appearance creosote has much depreciated within the last few years. That we do not get the same quality of material now that we did some years ago has been illustrated by Mr. Rolls, inasmuch as some poles have the appearance in a year or two of hardly having been creosoted, while others retain a perfect blackness, and no doubt this is in a measure due to inferior creosote. This is a question which I hope will receive the attention of gentlemen capable of dealing with it in the next session. It is one of great value, and will affect the success of creosoting as applied to telegraph poles.

I cannot pass over Mr. Graves's remarks with reference to the experience of Railway Engineers of this preservative. Mr. Graves admits that the evidence obtained from those gentlemen is somewhat conflicting. I do not doubt this, for I have myself seen decay in many sleepers; but this has not, in my opinion, been due to imperfect or improper creosoting, but to the practice of trimming the sleepers for the chairs, &c., after creosoting. Unless the timber is creosoted right through such treatment will probably leave exposed to the action of moisture some portion of the wood into which the oil has not penetrated. This is generally that part where the greatest solidity is required, viz., the seat of the chair. Wherever

creosoted timber is so exposed it should be well served with tar or pitch to prevent the penetration of moisture. It appears to me that it is not improbable the conflicting testimony spoken of is entirely due to this mode of treatment; if railway sleepers, fencing, &c., were trimmed and *bored* in all cases prior to creosoting, and the timber were at the same time free from moisture, I fully believe no decay would be observable.

I am all at one with Mr. Graves in advocating the extinction of moisture from timber prior to creosoting. All timber should be first properly seasoned to get rid of the sap, then kept dry. I am glad to have the support of so experienced an engineer on so important a point. The question is, no doubt, one of cost. Timber cannot be kept dry if stacked out of doors. Sheds must be provided, but they need be of no very elaborate construction; and, although they will undoubtedly cost something, the cost would not be great; and when the question is considered on its bearing with regard to the future duration of timber, and consequently heavy cost of renewal, I believe I shall not stand alone in advocating it.

Partial creosoting is doubtless not so good as entire creosoting; still it is creosoting, and I cannot adopt Mr. Graves's view in regarding it as an application of rarefied tar. Its application under the tanking arrangement is the same as when in the cylinder, except that there is no pressure. The heat applied to the tank liquifies the creosote and opens the pores of the wood into which the oil penetrates to a very fair depth, as is instanced by the specimens before the meeting. The system has many advantages. In many parts of the country the recognised system of creosoting is not obtainable. In such localities the timber may be cut down, trimmed, and stacked in the wood, and a tank set up and the butts creosoted as soon as the poles are properly seasoned. The tops of the poles would of course be protected by a coat or two of tar or paint; but, even if they were not, there can be no doubt the system will add largely to the life of the pole, for in protecting the lower portion from decay we protect that portion from which we suffer most.

There is, in my opinion, not the least necessity for "opening

out" creosoted poles. It is the influence of the sun, the warmer atmosphere, that draws out the creosote from the upper part of the pole, and causes it to trickle down to the ground-line. At this point this influence is lost. Below it the pole is fairly in antagonism with its surrounding medium, in the same manner as oil is with water. The oil is kept within the wood by the moisture of the soil; whilst the oil in turn keeps the moisture from penetrating the wood. Mr. Sach very truly remarks that if you keep the creosote in the pole above ground there is little fear of losing it lower down.

I agree that there is an advantage in allowing poles to remain in a horizontal or lateral position for some time after creosoting. What we require is for the oil to solidify in the pores of the wood—to, as it were, get hardened. It is then not so easily affected by the temperature, whereas, if used or set upright when newly done, that contained in the outer portion of the pole is easily drawn out by the warmer atmosphere. A good test of the settlement or solidification of the creosote in the wood may, I think, be obtained by lifting the pole from one end. If the oil is, as it were, still liquid, the pole will be elastic and supple. As it solidifies, so will the pole become more solid and less flexible.

This brings me to a statement of the flexibility of creosoted timber placed in my hands by Mr. Brain, Messrs. Bethell's manager. This statement may perhaps be found of sufficient interest to be published with the report of this discussion. Messrs. Bethell do not concur with the statement made in the paper, that all kinds of preparations tend to destroy the elasticity of the wood. I can only repeat such is my experience. Wood newly creosoted is no doubt as supple, or even more so, than prior to its subjection to this process; but in course of time the wood becomes *short*, as described. For this reason I would not advocate the *saturation* of poles prepared under this process for telegraph work. It would, in my opinion, be preferable to leave a good portion of the spine of the pole untouched by the creosote.

Mr. Granville has referred to a practice in vogue amongst farmers of applying boiled linseed oil mixed with charcoal dust

to the bottoms—that is, those portions placed beneath the soil—of gate-posts, which is said to be very durable. I take this as an argument in favour of the butt or tank creosoting process referred to, and, indeed, of the principle advanced in the paper, as the source from which the decay of all poles, not prepared under an oily process, decay. I should have liked to have seen this principle—this mechanical system of *disintegration*—more fully discussed. Does decay arise from it? We see the very same wood which we use for telegraph poles, and which, used as such, decays in seven or eight years, if kept dry—used for household furniture, or otherwise—last almost an interminable period. We have Mr. Haynes's evidence of the duration of iron-socketed poles. We have, further, Mr. Spagnoletti's evidence to the same effect. We have the all-acknowledged duration of creosoted timber; and we have Mr. Granville's evidence of a process which must come also under the head of an oil. All these are processes which exclude, and are antagonistic to, moisture. Has the antiseptic powers of creosote anything to do with the lasting properties which creosote imparts to timber, or is such derived merely from the oil itself? The evidence on the duration of iron-socketed poles is strongly in favour of the latter.

If we concede that the decay of telegraph poles proceeds from the absorption of moisture in that part which is placed in the ground our course is clear. Either we must creosote the pole entirely or partly, or else give it such protection as will exclude moisture from the portion buried in the soil. Beyond this we shall also have the opportunity of employing a cheaper description of timber than we have hitherto employed. There will be no reason why we should not employ home-grown Scotch fir, which is cheaper than that from abroad, and equally strong for our purposes as larch timber, though not so close-grained. It would more readily absorb the creosote; and where it could not be applied it would be equally well fitted for the application of a compound of pitch and tar.

I desire to tender my thanks to those gentlemen who have contributed to the interest of the paper by sending specimens of

decayed and other timber, and also to those who have taken part in the discussion which has been brought to bear upon it.

The CHAIRMAN said: It is difficult to over-estimate the importance of the discussion we have been carrying on. There can be no doubt that timber, on account of its rapid decay, forms the most important and costly item in telegraph maintenance. It has been assumed throughout the discussion that the decay to which we allude is the decay of the bottom of the pole. No one alludes to the decay of the upper portion, but that does occur on the western coast and in Scotland. Several processes for obviating that decay have been referred to—creosoting, sulphate of copper, boucherising, and kyanising—and the general opinion evidently is in favour of creosoting. Now timber may decay in two ways—one by eremacausis, the ordinary process of decay by the soil rotting the outside, and another by the action of the mycelium of fungi which grow in timber and cause it soon to become decayed. I see before me a piece of timber in that condition reduced to touchwood; and these metallic salts appear to act to prevent this decay in two ways. Some of them convert the albuminous product of the wood into a consolidated substance, and so take away the liability to decomposition; other solutions act to a great extent, and especially where the poles are planted on roads which pass through woods, in destroying the mycelia of fungi and preventing that slow internal rot or decay which would otherwise take place, and which causes the destruction of the poles. The great question is how creosote can best be employed. On this subject the remarks of Professor Abel as to the inferior quality of the creosote are very important. The subject, I may say, is one of special interest to myself, inasmuch as twenty years ago, as engineer of the Electric Telegraph Company, I made a great many experiments upon the preservation of poles. The use of the iron socket was a suggestion made by me to my brother; also the system of boring a hole up the pole and pouring creosote into it. I had supposed that experiment had been forgotten, and within the last year or two I have made some further experiments in that direction. I cannot help still attaching importance to it. I think if a pole is bored out 4 or 5 feet from

the bottom, kept filled with creosote, and securely plugged, which can be done at small cost, the results will be found to be important. In closing this discussion, I will ask you to return a cordial vote of thanks to Mr. Langdon for his extremely able and interesting paper.

Copy of letter addressed to Mr. W. H. Preece from Mr. W. H. Hyett, F.R.S.

Painswick House, Painswick, Stroud,
April 14th, 1874.

Dear Mr. Preece,

"I beg to thank you for the sight of the pamphlet on the Decay and Preservation of Larch Poles. It reminds me of a series of experiments I made on them many years ago. I had been assisting in a few experiments on a small scale to ascertain whether the leaves of plants inhaled or exhaled nitrogen like carbonic acid night or day, and I afterwards carried on similar experiments upon a greater scale to satisfy myself whether the light or heat of the sun, as compared with the darkness of night, gave out the sap as aqueous vapour in large quantities from the leaf. For this purpose I cut some twenty larch in the summer when the sap was running freely, and leaned them as perpendicularly as I could against the south side of a barn during twelve hours of the day, and then had them removed for twelve hours during the night within the barn, always taking care that the butt was pointed and rested on a board, so as not to draw any moisture from the ground, leaving on every bough and leaf. I weighed them, night and morning, for several days till all the leaves fell off. I have unfortunately mislaid the records of the experiments, but, if I recollect right, at the end of the first day they had all lost about 10 per cent. of their weight, and during the first night reabsorbed a quarter of what they had lost. On the second day they lost some 8 per cent. On the second night they regained some 2 per cent.; and this went on till all the leaves fell off, and no

further change of weight then took place. To all appearance, when the wood was immediately sawn out and planed it was as dry as if it had been cut in the winter and duly seasoned, and this I remember, that the specific gravity of a cube of 6 inches was less than that of the same sized cube of a larch that had been cut in the winter and exposed during the summer. The trees were about 25 or 30 years old. This led me to suspect that timber cut in the summer with all the boughs and leaves left on to evaporate the sap would have been at least as well seasoned as trees cut in the winter, if not better. I had intended to continue these experiments, especially the oak cut in the spring with leaves upon it and the sap rising sufficiently to bark the trees. This was intended to obviate the mischief of cutting oak-timber when the sap had begun to rise for the sake of the bark—but like many other contemplated experiments it was dropped—though I still think something might yet be made of it by young men who are up to the trouble of such trials. I am on the verge of fourscore. The author of the pamphlet does not seem to be aware of the elaborate experiments, on various antiseptics, described in the 'Annales du Chimie de Physique,' by M. Le Docteur Boucherie. If his 'boucherising' process refers to the same man, sulphate of copper was certainly not his nostrum; his experiments, though not mine, giving it greatly in favour of pyro-lignate of iron.

"I submitted the trees I experimented upon to fungus pits for some years, and Brunel used to send down and examine them frequently. Those treated with sulphate of copper decayed earlier than those treated with pyro-lignate of iron, and in all cases the strength of timber treated with metallic salts combined with mineral acids was much more injured from the very first than those which were treated with metals combined with vegetable acids; sulphates, and chlorides, and nitrates always made the wood brittle before decay, which the acetates and pyro-lignates did not.

"Yours very truly,

"W. H. HYETT."

Remarks handed in by Mr. Brain on behalf of Messrs. Bethell and Co. :—

Dryness is the principal quality required in timber that is to be creosoted. Timber is sometimes delivered dripping wet, and the creosoting firm expected to creosote it without delay. This is particularly the case with long timber, but I have been lately told by a Lancashire engineer that by standing sleepers delivered to him upright he has frequently got a gallon of water out of one. It is perhaps better that timber should not be creosoted at all than creosoted moist. In some cases the creosoting firm should be required to dry the timber in ovens, or, when this is not practicable, steam of about two atmospheres pressure should be driven through the cylinder, which will in a measure dry the wood.

As regards the want of elasticity in creosoted timber I venture to submit some opinions contained in very careful works by a Belgian and French engineer of Ponts et Chaussées. The former, M. Crepin, found that creosoted wood which had been some time in the sea at Ostend was remarkable both for its elasticity and hardness ; and M. Foresten, the French engineer, having remarked that M. Crepin's observations applied to wood some time creosoted and immerged in the sea, determined on trying experiments on creosoted wood soon after its leaving the cylinder. With regard to flexibility he says, "We loaded twenty-one pieces of oak, elm, beech, plane, red and white fir, Norwegian pine, Italian poplar, and white Dutch poplar, placing the weights on the middle of their long sides. They were $2\frac{1}{2}$ metres long between the points of support and 5 centimetres square. After having first loaded some pieces of each kind to breaking point we then ascertained the flexion of the twenty-one pieces under continually-increasing weights, but *not* up to breaking point; we then creosoted these pieces, and loaded them successively with the same weights, and we ascertained that their flexibility had sensibly increased."

The resistance to a crushing weight of creosoted and uncreosoted blocks is compared in a tabular statement by M. Foresten. On the Carolina poplar the creosoting seems to have diminished.

NATURE OF WOOD.	Number of blocks tried, both creosoted and uncreosoted.	Creosote injected per cubic feet.	Crushing weight		Difference.	
			for the uncreosoted bits.	for the creosoted bits.	Increased weight borne.	Less weight borne.
French Oak . .	8	120 k°	396 k	424 k	28 k	
Elm	12	285	388	392	4	
Ash	4	260	393	433	40	
Swedish Red Fir .	8	271	303	314	11	
Norway Red Fir .	12	319	418	432	14	
Dantzic Red Fir .	2	371	280	293	13	
Norway White Fir	4	205	392	400	8	
Plane	4	400	358	360	4	
Carolina Poplar .	9	545	270	258	—	12

"Belfast, 20th April, 1874.

"MY DEAR SIR,—

"I gave Mr. Fox (whom I think you know) the pamphlet on The Decay and Preservation of Telegraph Poles to read. I attach his remarks for your information. Hard larch in Ireland will take the boucherising process, owing, no doubt, to the dampness of the climate.

"I am yours very truly,

"M. C. GREENHILL.

"Geo. E. Preece, Esq."

"Belfast, 18th April, 1874.

"M. C. GREENHILL, Esq.

"I have perused the pamphlet on The Decay and Preservation of Telegraph Poles with much pleasure and satisfaction, and beg to detail my experience of the boucherising process in Ireland; and, as you are fully aware, from the quantity of poles which passed through my hands, different classes of timber were delivered, and for experiment I noted particularly the result of each kind; viz., Scotch fir, grown on soft peaty soil, were far more coarse and open, consequently more readily finished, than those grown on rocky soil. The average time taken to successfully boucherise 28-feet poles during the months of April, May, June, July,

August, and September was from five to seven days. In October, November, December, January, February, and March about ten days.

"I also endeavoured to ascertain the quantity of liquid percolated through three 28-feet poles in June and three of the same length in November. In the former 56 gallons were carefully measured, and in the latter 68 gallons.

"I also tried some few white or soft larch. These took a few days longer than the Scotch, but they certainly answer very well. A few of the red or hard larch were also tried. Some were successfully done, others when tested proved to have taken the solution but a very short way from the butt end. I reversed them and found the same effect at the small end.

"A small quantity of spruce and silver were also subjected to the process, and, as far as the boucherising of them, they answer well, but take much longer to do than either larch or Scotch; but I find these would not answer for telegraph purposes, as when dry they have a decided tendency to split.

"I am quite of the opinion that a large proportion of the larch grown in Ireland would answer well for boucherising, say four-fifths. They are certainly, with the Scotch, of a much coarser grain than I found in England, consequently could be boucherised quicker and at less expense.

"I am, Sir, your obedient servant,

"JAS. R. FOX."

The following Paper was then read:—

ON THE CHANGE OF THE RESISTANCE OF HIGH TENSION FUSES AT THE MOMENT OF FIRING.

By Major MALCOLM, R.E. Member of Council.

It had been my intention to have made this paper the subject of a communication to our Journal only, but it so happened that Professor Abel had promised a paper on electrical fuses, and it was

therefore thought that my communication might be advantageously read on the same night.

All that I propose to bring before you is the fact of the change of resistance of high tension fuses previous to and at the moment of explosion, and the degree of change, as far as I have been able in a few cases, to follow it.

It is no doubt well known to many who have been in the habit of using these fuses that their resistance changes according to the intensity of the current passing through them, on which account we at Chatham make it a rule to test them always with a battery of two Leclanché cells only, unless for any special reason, which is noted.

Working away to understand the action of these fuses as well as I could, I had often been struck with peculiarities, one of which I will here mention: viz.

Occasionally when I had a fuse in circuit with a delicate reflecting galvanometer of 1,360 ohms. resistance in bridge, I have noticed a regular vibration of the light, continuing for a considerable time, and as often as the current was turned on, so as to lead me and others to believe that the motion was due to no vibration of the room or other external cause, but solely to the varying passage of the current through the substance of the fuse, which seemed to discharge itself as if by the motion of some of its particles.

Now, thanks to the attention paid to the dynamo-electric machine by Messrs. Siemens, we have in a not very weighty form a machine, of which the last that has passed through my hands has been capable of firing 120 high tension fuses in continuous or plain circuit, and 110 in two parallel circuits. Another much lighter has fired 36 such fuses in six parallel circuits, showing that the spark was capable of a subdivision, which the Austrian frictional machine of Von Ebner is quite unable to approach. This machine also fired 49 fuses in continuous circuit.

I think we may take 100,000 ohms. as a fair average resistance of the ordinary high tension Abel land-service fuse made without any special precautions.

Some fuses greatly exceeding this limit, while others, as will shortly be seen, fall much below it, it became a matter of interest

to me to discover in what lay the tremendous power which enabled these dynamo-electric machines to overcome such enormous resistance with heat enough to explode the fuses; and, as we had no definite information at hand at Chatham, I was also anxious to find out the average number of cells necessary to insure the explosion of these fuses by voltaic electricity.

A hundred Leclanché cells size No. 3 were therefore prepared and a circuit arranged, commencing from one pole of the battery through a fuse whose resistance had been previously taken as before indicated, passing on through a Gaugain tangent galvanometer to the battery by a fork-tailed lead, so that the number of cells could be increased conveniently without breaking the circuit or originating disturbances of the needle.

The needle had been made very short and light, with a glass pointer fixed on to it.

We began always with one or two cells, and then by the forked tailed lead worked slowly up the battery, till the fuse exploded, noting carefully the deflection of the needle of the galvanometer until explosion. We then substituted such metallic resistance in the circuit as (with the number of cells which had produced explosion) reproduced the noted deflection.

The conduct of some of the fuses was strangely different from that of others; for instance, in the case of one with a resistance marked "Infinite," that is to say, one which gave no indication of a current from two cells, the resistance suddenly broke down before the current from 21 cells giving a violent deflection at moment of firing only.

But generally the deflection increased very fairly with the increased number of cells applied, as I tried to keep the influence of each degree of current power as nearly equal as I could, that is, I kept moving at a nearly equal speed; occasionally, however, the amount of deflection *fell.* I find one case noted, resistance 450. Number of cells to fire fuse 9. Deflection of galvanometer 5 (after giving a permanent deflection of 80).

The metallic resistance noted here as required to reproduce the deflection is 1,320 ohms.

In another case resistance 320, cells 8, deflection 5 (after deflection of 45), metallic resistance 1,440.

I am also aware of other instances of the same sort.

This is a variation of conduct exceedingly interesting to me, and I should be glad of a satisfactory explanation of it.

I may quote a few instances of the general law.

Initial Resistance of fuses.	Cells.	Deflection.	Metallic Resistance substituted to reproduce Deflection obtained just before firing.
490,000	18	1	17,000
15,000	10	11	750
499,000	16	16	640
425	10	6	1,350
1,550	5	15	265
9,550	35	10	3,750
10,500	39	8	5,500
10,500	38	10	3,990
37,000	43	10	4,950

It was because this breaking down of resistance seemed to me interesting in connection with some remarks made, especially by the President of this Society, on Mr. Preece's paper on Lightning Conductors, that I thought the record of an attempt to measure at least a portion of it might interest the Society.

What follows, besides explosion, after the resistance of a fuse has, by the energy of about 43 cells, been reduced from 37,000 ohms. to 4,950, I do not pretend to say; but, having watched the resistance fall down so low, it is, I think it fair to suppose that, in some cases with inconceivable rapidity, the resistance is still further broken down until the current can easily be conceived to be sufficiently great to heat the inflammable substances of which the fuse is composed to the exploding point.

On one occasion I tried if a microscope applied while a current was being passed though a fuse would show me any motion of particles. I only got a puff of smoke in my face.

Experiments with High-tension Fuses, 5th March, 1874.

Resistance of Fuses.	No. of Cells Fuse fired with.	Gaugain Tangent Galvanometer resist. 0·75 ohms.	Resistance just before firing.
490,000	18	1	18,000
15,000	10	11	780
420,200	20	Nil	
499,000	16	16	680
425	10	6	1,380
1,550	5	15	270
550	7	12	440
490,000	21	$3\frac{1}{2}$	5,250
450	9	5	1,350
680	10	10	840
4,400	15	$1\frac{1}{2}$	4,700
320	8	5	1,450
Infinite	21	Nil	
650,000	22	8	2,300
19,000	13	Nil	12,000

High-tension Fuses (Newest Pattern).

Resistance of Fuses.	No. of Cells Fuse fired with.	Deflection on Gaugain Tangent Galvanometer.	Resistance just before firing.
95,700 B.A.U.	35	10	3,750 B.A.U.
10,500 „	39	8	5,500 „
10,500 „	38	10	3,990 „
37,000 „	43	10	4,950 „
13,000 „	21	12	1,980 „
30,000 „	26	6	4,400 „
17,000 „	30	8	4,600 „
7,000 „	36	11	3,800 „

DISCUSSION.

The CHAIRMAN asked Major MALCOLM if he could tell them the nature of the compound of the fuse,—whether powder or solid.

Major MALCOLM replied that Professor Abel could give the best information on that subject.

The CHAIRMAN: Is it in the form of powder?

Major MALCOLM: Compressed powder.

Professor ABEL, F.R.S.: I might make an observation on this interesting communication which may throw some light upon it. The compound, both conducting and explosive, and offering a high resistance, in this fuse is a mixture of sub-phosphate of copper and sub-sulphide of copper and chloride of potash, which last is the explosive agent. The phosphate of copper offers high resistance, and in order to give the material increased conducting power it is mixed with sub-sulphide of copper. The mixture consists of these three components, mixed as intimately as possible by mechanical operation. This is pressed with a moderate degree of pressure round the poles of these high-tension fuses. Great care is required in the manufacture of these fuses, so as to make them sufficiently sensitive to be fired in large numbers by a high-tension apparatus such as this. The high-tension fuses have been made more carefully. They are well pressed until a certain deflection is obtained in the galvanometer, so that we have a clear indication of resistance as well as of tension. I believe it is in consequence of the fact of the variable compression of the material by mechanical means that the anomalous results referred to by Major Malcolm are produced. I should tell you that the indication furnished by the measure of resistance of the fuse is no indication of its sensitiveness to ignition. It does not follow, because it gives a low deflection of the galvanometer, it is more or less sensitive to ignition. The sensitiveness of the fuse is due to two causes: first, to the electrical conductivity of the fuse composition; and secondly, to the degree of compression applied in the production of the fuse. If

in a small quantity of the mixture, or in two small quantities of the same mixture, there exist a few particles more of sub-sulphide of copper in one than in the other, the one fuse will be electrically more conducting than the other; consequently the effect will be very different. On the other hand, the fuses will vary in sensitiveness, because one will contain more sub-sulphide of copper than another, and the examinations of the galvanometer will not correspond with the battery power required to explode the fuses. I cannot at the present moment explain the drop in the galvanometer at the time of explosion, but there can be no doubt that the variations with regard to sensitiveness to ignition and conductivity are due to two causes which operate in opposite directions.

Mr. W. H. Preece: I should be glad to hear how the resistance of the fuse is taken in the first instance, and whether this sudden failure of the galvanometer is noticed at the moment of explosion, and whether it continues after the explosion.

Major Malcolm, R.E.: I did not bring forward this question as offering to throw any light at all upon the irregularity of the fuses, for I do not think practically they are irregular; but, if I were to bring before you a table which will be printed with the paper, you would find they come down very equally. But the object I had in bringing it forward was, that some time ago there was a talk about atmospheric electricity, and I thought whether that had anything to do with the sudden break-down of resistance in fuses of great electrical power; and I found in this mixture—I do not care what its resistance may be—I found in this mixture the same sort of quality of resistance as in the fuse which I now bring forward. At Chatham we test the fuses with two cells of the Leclanché battery, and we balance with a Wheatstone bridge and reflecting galvanometer; and we found those fuses in which we suppose we do not put quite sufficient current through to explode them, balancing them immediately afterwards they will have a low resistance, and that resistance will rapidly rise. I do not say it will come back to what it originally was, because, being a mixture not prepared with special care, it is not likely the particles will rearrange themselves as they were; but, when you compare the tension

of one of these instruments as against the tension of an ordinary voltaic battery, it requires but a small portion of electromotive force to reduce the resistance, and that which puzzled me before I saw how the resistance fell seemed to be more or less explicable. [Mr. W. H. PREECE: When the needle falls?] The needle does not fall: the fuse explodes, and the circuit is broken. I think if I get those fuses of 490,000 resistances down to a resistance of 17,000, which I can trace, I look upon it as something like going down a slope. I know electricity acts with remarkable rapidity, and the action is too quick for the galvanometer to show it. Supposing the resistance falls to zero, it must be done in such a small portion of time that I have neither materials nor knowledge which will enable me to follow it at present.

Professor ABEL, F.R.S.: There is one point I would lay before you, and that is, as to the development of heat. As the heat is raised the conductivity of the material is increased. When that reaches a certain point in one particular fuse, that fuse, while the needle is still falling, explodes. The resistance may be less in one fuse at the moment of explosion than in another, because the amount of heat required to explode one fuse will be less than that in another, and you will have a different fall of the galvanometer at the moment of explosion in one fuse than you have in another. Major Malcolm has endeavoured to measure the fall of the galvanometer till the moment when the fuse explodes, and what he registers is the deflection at the instant of or just before the explosion, and that varies with the amount of heat developed and the sensitiveness of the material to be exploded by heat, which will be variable.

Mr. W. H. PREECE: I think Major Malcolm has started an interesting branch of inquiry. He alluded to some observations that were made in a paper which I read before this Society on Atmospheric Electricity, and in that paper I endeavoured to point out that one of the causes which induced lightning protectors to act in the way they did was, that at the moment when a flash of lightning struck across a layer of air it converted that path into a line of no resistance. Hence the whole of the current due to atmospheric electricity passes along this small line of no resistance in

preference to going through a coil of the instrument which possesses some resistance. Since then I have endeavoured to pursue this inquiry and to trace it out, but want of time has prevented me from putting into shape the results of a great mass of experiments on the point, the experiments being made to watch the effect of increased electromotive force upon the resistance of an insulating medium, the same as Major Malcolm has endeavoured to show. The experiments were in this way: Layers of air of various thickness and density were arranged by means of tubes and air-pumps. Electromotive force was applied from one cell up to 6,000 cells, and by that means a series of experiments were made which show that in the case of any insulating medium we find that, as we increase the electromotive force, so we reduce the resistance of the body opposed to the current. In the case of air, it follows definite laws which I hope to bring before you. In the case of fuses, I have no doubt it follows a similar law. Most of us know that in cables we get varying results according to the power we test, and the higher the power the less is the result that is obtained. We therefore have this fact —that as we increase the electromotive force applied to test either air, or gutta-percha, or india-rubber, or any insulating medium, we find the resistance diminishes. If Major Malcolm tries this in his fuses, and others who have the opportunity of testing will do the same, in a short time we shall be able to find out definite laws, which will be a great addition to our science—that is, the laws which determine the resistance of bodies which *oppose* the passage of currents. We know already the laws which apply to bodies which *favour* the passage of currents.

Professor FOSTER, F.R.S.: I would beg to suggest whether it is possible that the explosion of these fuses may to a certain extent be due to a different action to that of heat—the composition of substances which react upon each other chemically with great energy—and it does not seem to me impossible that the conduction may be of the nature of chemical conduction. The current is conducted not only by the sulphide of copper, but there may be some kind of polarisation of the particles tending to make them act chemically towards each other. It struck me that one possible mode of action,

in addition to heat, may be to commence to set up chemical action, which once set going may go on increasing, and a small spark may produce great combustion.

Professor ABEL, F.R.S.: I have no doubt such action as Professor Foster describes takes place. One material will act upon the other. No doubt when once heat is developed to a sufficient extent chemical action begins to take place, and in the proportion in which that takes place it is an assistance in the reduction of the resistance of the fuse. No doubt the action is twofold, viz., the development of heat and the development of chemical action. That no doubt explains the fact that when these fuses have been submitted to this severe ordeal they seldom return to the same condition after that time.

The following Paper was then read:—

NOTES RELATING TO ELECTRIC FUSES.

By PROFESSOR ABEL, F.R.S. Member of Council.

IN an interesting paper on the "Electrical Ignition of Explosives,"* communicated to the Society by Lieut.-Colonel Stotherd, R.E. in May, 1872, that Officer pointed out that the earliest practical application of electricity to the explosion of submerged charges of gunpowder was made by Sir Charles Pasley, who, in the course of operations which he was carrying on for removing the wreck of the Royal George at Spithead, consulted the late Professor Daniell and Sir Charles Wheatstone, at King's College, on the subject, the result being the employment by him of a form of Daniell's battery for the purpose of heating to redness, or fusing, a platinum wire surrounded by gunpowder, an expedient which appears to have been first successfully applied a few years previously by French military engineers.

From that period until about 1860 the wire-fuse was generally

* Journal of the Society of Telegraph Engineers, vol. i. p. 209.

regarded in this country as the only reliable means for exploding mines by electric agency, and considerable attention was given to the elaboration of appliances suitable for carrying out such operations by the Royal Engineers, more especially by Colonel Ward, who published a most instructive memoir on the subject in 1855,* and was led, by systematic experiments, to construct a form of Grove's battery as the most suitable exploding agent for mining operations.

In 1856 the subject of the application of electricity to the explosion of gunpowder was referred for experimental investigation to the Ordnance Select Committee, of which Colonel Bainbridge, R.E., Sir Charles Wheatstone, and I were then members. The practical inconveniences attending the employment of large voltaic batteries in military operations, and the necessity for providing great excess of power in consequence of the want of constancy of batteries suited to the ignition of platinum (or iron) wire fuses, gave direction to the investigations which were commenced by that Committee, and continued by Sir Charles Wheatstone and myself.

The possibility of successfully employing electricity of high tension as the exploding agency in extensive mining and military operations had already been demonstrated by Moses Shaw, by Warrentrap and Gätzmann, by Charles Winter, by Verdu, du Moncel and Savare, and by Von Ebner.† The latter, an eminent officer of the Austrian Engineers, who was one of the earliest to apply a well-organised system of submarine mines to the defence of harbours, had constructed in 1855 a frictional electric machine of glass, with Leyden jar and desiccating arrangements, specially designed for the explosion of mines in military operations. This

* Inquiry into the application of the voltaic battery to military purposes, by H. Ward, Capt. R.E. Professional Papers of the Corps of Royal Engineers, vol. iv. New Series, p. 113.

† The employment of frictional electricity as an agent for exploding gunpowder first suggested itself to Franklin in 1751 and to Priestley in 1767. Franklin, in his "Letters on Electricity," (June 29th, 1751,) describes the precise method of operation to be pursued.

was, however, soon superseded by a very portable and powerful frictional machine, constructed of ebonite, under Von Ebner's instructions, by Lenoir of Vienna and Messrs. Siemens of Berlin, which was first introduced into this country at the International Exhibition of 1862, and was adopted for a time by the Royal Engineers as the service exploding instrument for field operations.

The investigations commenced by Sir C. Wheatstone and myself in 1856 had reference to the further development of the application of high-tension currents, as obtained with electro-magnetic induction coils and frictional machines (including Armstrong's hydro-electric machine), to the explosion of mines, and especially to the utilisation of magneto-electric induction apparatus in the same direction.* The production of a highly sensitive and otherwise efficient fuse for employment with such instruments, as well as with other high-tension apparatus, was made the subject of special investigation by me, in which I received most valuable assistance from Mr. E. O. Brown. After applying gun-cotton and various other highly sensitive compounds and preparations as the priming (or vehicles for ignition) of such fuses, the ultimate result was the production of the high-tension phosphide of copper fuse specially referred to in Colonel Stotherd's paper, through the agency of which even small voltaic piles and so-called galvanic chains, as well as the simple forms of magneto-electric machines manufactured for medical purposes, became available for the explosion of mines. A very efficient magneto-electric exploding machine was also devised by Sir C. Wheatstone, by means of which a large number of those fuses could be exploded in divided circuit with such rapidity as to have the practical effect of simultaneous explosions in operations *on land*, and which served as the foundation for the construction of other instruments of the same class, though differing somewhat as regards the quality of the currents they developed, such as those of Beardslee, Breguet, Browning, Marcus, and Ladd. Although the highly sensitive character of the

* *Vide* Report to the Secretary of State for War on the application of electricity to the explosion of gunpowder, by Wheatstone and Abel, Nov. 1860. Eyre and Spottiswoode, 1861.

phosphide of copper fuse, the construction of which has been sufficiently described to the Society in Colonel Stotherd's paper, has rendered it very valuable, and superior to other fuses of similar nature, such as those of Statham, Von Ebner, and Beardslee, as a means of exploding mines and groups of mines, both land and submarine, its very sensitiveness became an obstacle to its employment with confidence in *submarine* mines, as pointed out by Colonel Stotherd. I was therefore recently led to devise a modification of the high-tension fuse, specially designed for use in submarine mines, which has also been described in Colonel Stotherd's paper, and which, while possessing the requisite sensitiveness to ignition, has been proved by ample experiments to sustain with absolute safety the transmission of any test-currents to which it may be necessary or desirable to submit them.

Military engineers and others were not slow to avail themselves extensively of the advantages afforded by the sensitive high-tension (phosphide of copper) fuse in dispensing with the necessity for employing cumbrous and troublesome batteries, and in rendering the success of exploding operations independent of distance and of good metallic connections. While, however, these important facilities which the tension fuse afforded were accepted somewhat as a matter of course, some failures, experienced with them from time to time, chiefly in consequence of the application of particular forms of them to uses for which they were not designed, have led to their somewhat hasty condemnation by some who gladly benefitted in the first instance by their valuable properties, and who have ascribed altogether to a want of stability and trustworthiness of the fuse results which were at any rate in great measure ascribable to a want of sufficient foresight or consideration on the part of those who employed them. Thus the high-tension *gun-tube*, which was specially, and it may be said exclusively, devised for the proof of cannon at Woolwich and for employment in experimental artillery practice in this country, is of very simple and light construction, consisting of a porous wooden head containing the electric fuse itself, with a quill charged with the priming powder fitted into it, without any special care. Such a contrivance is obviously not at

all adapted to resist penetration of moisture to the interior if kept in a damp atmosphere for any length of time, and, although the ingredients of the phosphide priming composition* inclosed in the wooden head are stable in themselves, and have as little tendency to change when intimately mixed together, *the mixture being kept dry*, the absorption of moisture by it must inevitably result in some chemical change, which in time prejudicially affects, and may altogether destroy, the original properties of the material. In the same way, the detonating composition in *friction* gun-tubes has been found to have become completely deteriorated by their being kept in damp climates or localities, there being this difference between the two, that, as the electric priming composition is of a comparatively delicate nature, it is the most rapidly affected. Now, these electric gun-tubes were adopted for the simultaneous firing of guns on board ship, without any question as to whether their construction was of a nature to enable them to resist the effects of damp to which they must necessarily be subject at sea and in different climates; the consequent occurrence of some failures with them, which were however by no means always clearly traced to the gun-tubes themselves, has resulted in the expression of some strong opinions adverse to the general efficiency of the high-tension fuse. The same description of electric gun-tube, manufactured for use in the firing of time-guns, appears to have found its way to America for employment in submarine mines during the war, probably without even the adoption of the obvious precaution of preserving the tubes in properly constructed packages; and because, after several years, these are found to have become useless, they are referred to as affording absolute proof of the unfitness of tension fuses for employment in submarine mines.† While there is no question that the priming-mixture used as the igniting medium in a phosphide high-tension fuse is liable to deterioration if moisture have access to it, there is no difficulty in so con-

* The subphosphide of copper and subsulphide of copper are prepared at high temperatures, by special processes, by which their stability is secured.

† Paper by N. J. Holmes on Torpedoes, read before the Society, 11th February, 1874.

structing the fuses or gun-tubes themselves as to render the penetration of moisture to the interior almost impossible; hence the objection raised against their employment because certain fuses have suffered from the effects of climate in no way affects the real question of the permanent efficiency of a properly constructed tension fuse. It is obvious that, in an electric fuse constructed of platinum or other wire, the primary igniting agent namely, the wire, is not at all susceptible of deterioration by the effects of moisture, but the same cannot be said of the gun-cotton preparation or other sensitive explosive substance with which it is necessary to surround the wire in order to render the fuses sufficiently delicate for present requirements; hence it is equally essential to the permanent efficiency of low-tension or wire fuses that they should be carefully constructed with the view of precluding the possibility of penetration of moisture to their interior, otherwise they will certainly be found to exhibit a similar liability to deterioration to that for which high-tension fuses have been condemned in several quarters.

A really valid objection to the employment of the phosphide of copper tension fuse in submarine mines has, however, already been referred to, as pointed out by Colonel Stotherd, namely, its liability to considerable alterations in conductivity by being frequently, or for long periods, submitted to even feeble test-currents, and the possibility of its accidental ignition by passing through it such comparatively powerful test- or signal-currents as it may be found necessary to employ in conjunction with an efficient system of submarine mines. The new tension fuse, specially devised by me for use in submarine mines, is, as already stated, quite free from the latter defect, and, though its conductivity is by no means unaffected by the long-continued transmission of test-currents through it, the changes to which it is liable in this respect have been found, so far as present experience goes, not to affect its efficiency.

A second objection, the validity of which can scarcely be doubted, though it has perhaps not yet been sufficiently established by positive results, applies to all descriptions of fuses arranged for

ignition by high-tension electricity. This consists in a possibility of their accidental ignition by currents induced in insulated cables during thunderstorms or less violent atmospheric electrical disturbances. It has been amply demonstrated by experiment, and by results obtained in military service operations, that if insulated wires immersed in water, buried in the earth, or even extended on the ground, are in sufficient proximity to one another, each cable being in circuit with a high-tension fuse and the earth, the explosion of any one of the fuses by a charge from a Leyden jar, or from a Dynamo-electric machine of considerable power, may be attended by the simultaneous ignition of the fuses attached to adjacent cables, which are not connected with the source of electricity, but which become charged by the inductive action of the transmitted current to a sufficient extent to produce this result. Such being the case, it appears very possible that insulated cables extending to land or submarine mines, in which high-tension fuses are inclosed, may become charged inductively during violent atmospheric electrical disturbances to such an extent as to lead to the accidental explosion of mines with which they are connected. Mr. G. E. Preece, in the interesting paper on underground telegraphs which he communicated to this Society,* gives an instance of the inductive effects of lightning discharges upon underground cables inclosed in pipes, the persons engaged in the operation of jointing the wires during a storm having seen sparks pass between the bare joint of the wires and the joint-box against which they were resting. In a discussion which followed, I believe, upon that paper, Mr. Varley and Mr. Latimer Clark both referred to personal experience of the inductive charging of cables, both underground and submarine, and, although the lengths of cables used in mining operations are quite insignificant when compared with the shortest telegraph cables to which the observations of those gentlemen refer, the sensitiveness of high-tension fuses to ignition fully justifies the doubts at present entertained whether their use may not be attended by a possibility of serious risk of accident, or, at any rate, of the unintentional explosion of mines placed in position for purposes of defence, especially in

* Journal of the Society of Telegraph Engineers, vol. ii. p. 387.

climates where very violent electrical disturbances are of frequent occurrence. Apprehensions of this nature were entertained by Baron von Ebner, and in a Report by that Officer on the defence of Venice, Pola, and Lissa, by submarine mines, in 1866, he refers to the accidental explosion of one of a group of sixteen mines during a heavy thunderstorm, as well as to the explosion of some mines in the harbour of Pola, by the direct charging of the cables, through the firing station having been struck by lightning. It was to avoid such accidental explosions that he devised an ingenious, but complicated, circuit-closing arrangement, to be applied in the submarine mines themselves, by the employment of which the fuses in the mine were only brought into connection with the cable leading to the firing stations when the mine was struck by a passing ship.

Two instances of the accidental explosion of tension fuses by the *direct* charging of over-head wires during lightning discharges occurred last year at Woolwich, and a fuse connected with an over-head insulated wire at Chatham was also exploded accidentally last summer, though whether by an induced charge or by the direct action of a lightning discharge was not conclusively demonstrated. Careful arrangements have therefore been made at the School of Military Engineering for the purpose of obtaining thoroughly reliable evidence on this most important subject, and the observations which it is hoped to record will probably serve in great measure to decide whether it is desirable that the submarine tension fuses now in use in England in connection with torpedo service should be replaced by low-tension or wire fuses.

The latter present some special advantages in the application of electricity to the simultaneous explosion of a number of guns on board ship, particularly as the system of firing most readily susceptible of efficient application is by branch circuits. The subject of the construction of thoroughly efficient low-tension fuses and gun-tubes has therefore, of late, received careful attention by myself and other Government officials engaged in the elaboration of details connected with the service-applications of electricity, and some results which I have obtained in the course of comparative experiments on the electric resistances and other properties of wires of different materials, chiefly with the view to ascertain their

relative suitability for employment in electric fuses, have been considered by some of my colleagues of sufficient general interest to warrant their communication to this Society.

The highly permanent character of platinum, combined with the comparatively great resistance which it opposes to the passage of an electric current, has led to its being almost exclusively used until quite recently as the material for the construction of the bridge, or the source of heat, in low-tension fuses. The wire of this material hitherto used in the production of fuses for military purposes has varied somewhat in diameter, and the resistance offered by it is not very high. Its average diameter is about 0″·003, and its weight 1·65 grains per yard; its resistance is equal to about 31 Ohms. per yard; so that the length of wire which has generally been introduced into the construction of a fuse has offered a resistance of about 0·2 Ohm.

With the view of ascertaining how far the sensitiveness of low-tension fuses could be increased by employing thinner wires of platinum, two, much finer than the above, were procured; one of these had a diameter of 0″·0012, and weighed 0·4 grain per yard; the other, which was the finest wire procurable with existing appliances, weighed 0·265 grain per yard, and had a diameter of 0″·0009. Some fuses were prepared very carefully with 0″·2 lengths of each of these wires, and with a sample of the coarser wire ordinarily used. The resistances of these fuses were found to be as follows:—

Description of Wire.	"Service" Wire.	Medium Wire.	Finest Wire.
Diameter	0″·003	0″·0012	0″·0009
Weight per yard . .	1·65 grn.	0·42 grn.	0·265 grn.
Resistances of fuses.	0·2 0·2 0·21	0·4 0·4 0·3	0·77 0·75 0·72
	0·2 0·2 0·2	0·4 0·4 0·4	0·65 0·70 0·60

Employing as the firing battery 10 cells of a modified form of the Leclanché battery, constructed at the Silvertown Telegraph Works, the following results were obtained:—

	"Service" Wire.	Medium Wire.	Finest Wire.
One fuse just ignited gun-cotton through	9 Ohms.	23 Ohms.	30·35 Ohms.

Lengths of 0″·3 of these wires, stretched across supports of brass, furnished the following results:—

	"Service" Wire.	Medium Wire.	Finest Wire.
The 10-cell "Silvertown" battery fused	2 lengths in divided circuit through 1·8 Ohm.	4 lengths in divided circuit through 2 Ohms.	8 lengths in divided circuit through 2 Ohms.

The foregoing results show that the "medium" wire already offers important advantages over the "service" wire when applied as a bridge in fuses, and that, approximately, about four times the amount of work in the desired direction can be performed by a given battery, and under given conditions, through the agency of the finest wire, than is achievable by means of the service wire.

It was found decidedly less easy to make up the finest wire into fuses than the thickest wire; no great difficulty was, however, experienced, after short practice, in attaching it to the fuse-terminals with the aid of solder. It was, however, not so easy to insure uniformity in the length of wire used in a fuse by straining the finest wire across the terminals, as is shown by the comparatively great variations in the resistance of the finest wire fuses. There was also comparatively great risk of breaking the very fine wires in attaching the gun-cotton priming to them. Still, these difficulties did not appear so formidable as to constitute vital objections against employing the very fine wire.

In continuing the experiments with fine platinum wires, some apparently anomalous results led to the observation that the wire, of nearly the same diameter, supplied at different times by one and the same maker (Messrs. Johnson and Matthey), presented very marked differences in conductivity. Several specimens of wires of various diameters, received at different times, were submitted to comparative examination; one of these being taken as a standard for comparison, the following differences in their electrical resistances were observed:—

No. of Specimen.	Weight in grs. per yard.	Resistance per yard.	Calculated resistance taking No. 1 as standard.	Difference.
1	0·268	136·7 Ohms.	136·7 Ohms.	
2	0·280	147 „	130·9 „	+ 16·1
3	0·36	126 „	101·8 „	+ 24·2
4	0·41	100 „	89·3 „	+ 10·7
5	0·94	52·2 „	39·0 „	+ 13·2
6	1·33	42·9 „	27·55 „	+ 15·35
7	1·34	31·4 „	27·34 „	+ 4·06
8	1·65	21·15 „	22·20 „	− 1·05

It will be seen from these numbers that the variations in the resistance of the metal are very considerable; thus the resistance of No. 2 wire should be somewhat lower than that of No. 1, which is a slightly finer wire, but it is actually 5·8 Ohms. per yard higher, and a still greater difference in the same direction exists between wires 2 and 3. Nos. 6 and 7, which are very nearly alike in weight, also differ very considerably from each other in conductivity. No. 6 weighs only 0·01 grn. per yard less than No. 7, but its resistance is 11·5 Ohms. per yard higher. Again, No. 6 weighs only 0·32 grn. per yard less than No. 8, but its resistance is almost double that of the latter. Although it was obvious that such great differences as these could not be due to the wires having been annealed in some cases and not in others, the effect of annealing was tried upon Nos. 6 and 7; the resulting alteration in resistance was nearly the same in both cases, that of No. 6 being reduced 0·4 Ohm. and that of No. 7, 0·5 Ohm. per yard.

These great differences in the conductivity of different platinum wires might obviously be due to one or other, or to a combination, of two causes, namely, a difference in the purity or in the structure of the metal. The quantities of the different wires at my disposal were insufficient for me to attempt any comparative chemical examination of them, but I am informed by Messrs. Johnson and Matthey that very decided variations in the degree of purity of the metal ordinarily used for conversion into wire may undoubtedly exist; and when it is borne in mind that one of the metals associated with platinum, and the one perhaps most difficult of complete removal, namely, iridium, reduces its conducting power in a remarkable degree, it may not be difficult to account for the variations in the electric resistances of wires produced at different times, by a difference in chemical quality alone. In order to ascertain, however, whether those variations might also be ascribable in some measure to differences in the structure or density of the metal, I obtained from Messrs. Johnson and Matthey some samples of platinum wire drawn from the fused metal and from forged platinum-sponge (or precipitated metal), the latter being the material always employed in the production of wire. The differences between bra-conductivity of these wires was remarkable, that of the wires

drawn from one and the same piece of fused metal being little more than half the conductivity of the wires made from forged sponge. On comparing their electric resistances with that of No. 1 specimen in the table just now given, the numbers are as follows:—

	Weight in grs. per yard.	Resistance per yard.
No. 1 specimen (annealed)	0·268	136·7 Ohms.
Wire from forged sponge (annealed) . .	0·268	126·5 „
Wire from fused metal (annealed) . .	0·268	228·1 „

The resistances of hard-drawn wires from the same specimens of metal were slightly higher.

The above difference is no doubt to some extent ascribable to the difference between the specific gravities of the fused metal and the forged sponge. Deville and Debray assign to fused platinum (not forged) a density of 21·15, while that of hammered platinum is fixed (by Wollaston) at 21·25; but the specific gravity of platinum drawn to very fine wire is stated on the latter authority to be 21·5; and, although this number is probably too high, being obtained a good many years ago (when the separation of platinum from its associate metals was but imperfectly elaborated), there can be no question that the process of drawing into fine wire must condense the metal to so considerable an extent as, at any rate, greatly to reduce the difference with respect to density and consequent conductivity between the fused metal and the forged sponge.

With a view to ascertain whether the electrical resistances of platinum wires produced from sponge-metal of the same degree of purity, but forged to different extents, presented any important differences, Messrs. Johnson and Matthey very obligingly prepared for me the following samples. A piece of soft welded platinum of high chemical quality, measuring $3'' \times 2'' + 1''$, was forged into a piece about 6 inches long and 1 inch square, weighing between 70 and 80 ounces. One-third of this bar was drawn into wire No. 1, whilst the remainder was reduced by forging to a bar $0''{\cdot}5$ square; part of this was drawn into wire No. 2, and the other portion was submitted to further forging, till it was reduced to a bar $0''{\cdot}25$ square, from which a wire No. 3, corresponding in

diameter to the two others, was drawn. The resistances of these wires were compared, and the specific gravities of the three differently forged pieces of the same metal were determined by means of large samples (weighing about one pound each), kindly lent me for the purpose by Messrs. Johnson and Matthey. The numbers obtained were as follows:

	Diameter of Wire.	Resistance per yard.	Specific Gravity of the forged samples.
No. 1	0″·0225	0·453 Ohms.	21·32
" 2	0″·0225	0·449 "	21·34
" 3	0″·022	0·452 "	21·32

From these results it would appear that very considerable differences in the amount of forging which the metal had received did not affect, to any important extent, either its specific gravity or the conductivity of the wire drawn from it. It is somewhat curious that No. 2 sample should be slightly denser than No. 3, which had been more highly forged, but it is also interesting to observe that this slight difference in density accorded with a difference in conductivity between Nos. 2 and 3. When portions of the same samples of metal were drawn to much finer wires this slight difference disappeared, as shown by the following numbers:

	Diameter.	Weight per yard.	Resistance per yard.
No. 1	0·0014	0·42 grs.	87·5 Ohms.
" 2	0·0014	0·42 "	87·5 "
" 3	0·0014	0·42 "	87·5 "

The sample of *fused* metal, from which the wire just now spoken of as presenting a very high resistance had been prepared, was found to have a specific gravity of 21·09, a number which would apparently account completely for the low conductivity of the wire produced from this metal. As, however, the density was decidedly lower than that assigned to fused platinum by Deville and Debray (21·15), who doubtless experimented with a very pure sample of metal, I was disposed to believe that the low result was in part due to impurities in the platinum. Mr. G. Matthey, while coinciding in this opinion, furnished me with a wire prepared from absolutely pure sponge, melted. This metal had been submitted to considerable forging—to my great regret, as its original specific gravity could

not be ascertained; that of the forged mass (a piece weighing about 2 lb.) was 21·39, being therefore slightly above that of the samples of forged sponge of high commercial quality. The resistance of the sample of wire from this pure metal was decidedly, though not greatly, higher than that produced from the forged sponge of very good commercial metal, although the specific gravity of the latter was somewhat the lowest, as is shown by the following numbers:

Description of Wire.	Diameter.	Weight per yard.	Resistance per yard.
Made from forged sponge of high commercial quality .	0″·0014	0·42 grs.	87·5 Ohms.
Made from pure fused sponge .	0″·0014	0·426 „	88·9 „
Obtained from the makers in the ordinary course . .	0″·0014	0·41 „	89·3 „

I have added to this comparative statement the results furnished by one of the wires, supplied in the ordinary course, which appeared to be similar in purity (as regards the existence of iridium) to the experimental wires prepared from the forged sponge. It will be seen that the wire in question is somewhat less heavy than either of the other wires, and that its resistance is also somewhat higher.

It appears to be clearly established by the foregoing experiments that the conductivity of such *fine* platinum wires as are employed in the construction of electric fuses is but slightly affected by physical differences in the metal from which they are produced, and that the considerable variations observed in the electric resistances of commercial samples of fine wires are ascribable to variations in the chemical quality or degree of purity of the metal.

In the course of my experiments, bearing on the application of fine wires to the production of comparatively sensitive low-tension fuses, it was brought to my notice by Lieut. Bucknill, R.E., that Mr. Farmer, of the United States, who has recently devoted much attention to the improvement of appliances for the explosion of mines, had applied German silver wire to the construction of low-tension fuses with very promising results. I therefore procured from Messrs. Johnson and Matthey a fine wire of that material for preliminary experiment. The diameter of this wire was 0″·0012,

and it weighed 0·14 grn. per yard. Six fuses, fitted with 0″·2 of this wire, presented the following resistances: 1·15, 1·2, 1·3, 1·34, 1·3, 1·4. One such fuse just ignited gun-cotton through 35·40 Ohms., with employment of the 10-cell "Silvertown" Leclanché battery; and eight lengths of 0″·3, in divided circuit, fixed between brass pillars, were fused through two Ohms. This wire, therefore, ranked about equal, as a material for fuses, to the finest platinum wire experimented with. I also made some preliminary experiments, furnishing about equally promising results, with a wire of the alloy of about 33 parts of platinum with 66 of silver, which was adopted by Matthiessen for the reproduction of the standard of electrical resistance. The idea of applying this alloy to the construction of low-tension fuses was first entertained by Commander J. Fisher, R.N., who was led to expect, from Matthiessen's published statements, that its electric resistance would be greatly superior to that of German silver, which were, however, not borne out by the comparative experiments instituted.

With the view of ascertaining whether wires of either of these alloys presented decided advantages over fine platinum wire as a material for the construction of low-tension fuses, I obtained from Messrs. Johnson and Matthey two series of wires, the one consisting of wires of equal diameters, of platinum, German silver, and platinum-silver, corresponding to the "medium" platinum wire previously experimented with, the other series also including wires of the three materials, but of as small a diameter as could be procured with existing appliances. These wires were submitted to experiment, which it was endeavoured to render pretty strictly comparative, though it was difficult to guard against occasional discrepancies due to slight fluctuations in the power of the batteries used in successive experiments.

The diameters, weights per yard, and electrical resistances of the several wires having been ascertained, it was next determined through what resistances one or more lengths of 0″·3 of each wire, supported between brass pillars, were fused, or heated short of fusion, or to a sufficient degree to ignite gun-cotton, by means of two forms of the Leclanché battery, both coupled up for tension;

one being the "Silvertown" pattern already referred to, and the other a battery constructed for the special purpose of firing a large number of fuses simultaneously through divided circuits of different lengths. The results obtained are given in Table I.

The conclusions arrived at from these experiments are as follows:—

1. Both the German silver and the platinum-silver alloy are greatly superior to platinum in regard to the resistance opposed to the passage of a current, and the heat consequently developed in giving lengths of wires of a particular diameter.

2. German silver is, in its turn, superior in this respect to the platinum-silver alloy, as is especially apparent in the finest wires. This superiority, though very marked in considerable lengths of wires of the same diameter, is, however, only trifling in short lengths; the comparatively ready fusibility of the platinum-silver wire contributing, with other physical peculiarities of the two alloys, to reduce the fine German silver wire to about a level with it, and sometimes even to a somewhat lower level, as regards the thermal effects produced. Some slight influence in this direction may perhaps also have been exerted, in these particular experiments, by the greater cooling power exerted, by the brass supports between which the wires were fixed, upon the German silver wire of comparatively low specific gravity.

In selecting a wire for the construction of low-tension fuses, its power to resist corrosion when in intimate contact with gunpowder, or other substance employed as the igniting medium in such fuses, especially if some moisture should be accidentally absorbed by them, possesses an important bearing upon its permanent efficiency. In Matthiessen's Report on the application of platinum-silver wire to the preparation of standard resistance-coils, it is stated not to oxidise by exposure to air; it has, however, been observed to do so gradually, to the extent of becoming tarnished, and as, with extremely fine wires, a very superficial oxidation might appreciably affect the resistance of fuses constructed with them, it was thought

desirable to ascertain by actual experiment, of a somewhat severe nature, the power of such wires to resist corrosion in comparison with German silver wire. It was, of course, unnecessary to experiment with platinum wire in this direction. Wires of the two alloys, about eight inches long, were inclosed in glass tubes, and each wire was surrounded closely by gunpowder and gun-cotton in the ordinary air-dry condition; one of each being also simply inclosed in air, for purposes of comparison. In addition, a length of about two inches of each kind of wire was buried in wet gunpowder, the extremities being allowed to protrude sufficiently for testing purposes. The resistances of these specimens were periodically examined, and the numerical results of those observations are given in Table II.

The slight fluctuations exhibited in two or three instances in the resistance-measurements of the fine and comparatively light German silver wire in the foregoing observations are no doubt ascribable to the effects of fluctuations of temperature, which did not influence the readings obtained with the platinum-silver wire, its conductivity being less affected by changes of temperature than that of any alloy experimented upon by Matthiessen. In all three experiments with the German silver wires inclosed in tubes there were indications of very slight superficial oxidation, as demonstrated by the permanent, though small, increase in the resistance of these wires, which, in the case of its exposure to contact with gun-cotton, continued very gradually up to the forty-first day, when it became stationary. The resistance of the platinum-silver wires in glass tubes remained constant throughout.

The exposure of the wires to contact with *wet* gunpowder has afforded very marked evidence of the greater liability of the German silver wire to corrosion; at the same time it must be borne in mind that this test far exceeds in severity any conditions under which fuses constructed with wire bridges would be likely to be preserved in actual practice. It will be seen that the platinum-silver wire remained unaffected by this test thirty-two days after the continuity of the German silver wire had been destroyed by corrosion. The great influence of the charcoal in the gunpowder

Table I.

	Finest Wire which Messrs. Johnson and Matthey could furnish. *See* page 276.			Wires corresponding in thickness to the *medium* Platinum Wire. *See* page 276.		
Description.	Platinum.	Platinum Silver.	German Silver.	Platinum.	Platinum Silver.	German Silver.
Diameter . . . . . Weight per yard in grains .	0″·001 0·28	0″·001 0·17	0″·0011 0·11	0″·0014 0·41	0″·0014 0·21	0″·0014 0·15
Electrical resistance in Ohms Per yard . . . . „ inch (calculated) . „ 0″·3 . . . . „ 0″·25 . . . . „ 0″·20	 147 4·1 1·23 1·02 0·82	 296 8·2 2·46 2·05 1·64	 338 9·4 2·82 2·35 1·88	 100 7·8 0·84 0·70 0·56	 237 6·6 1·98 1·65 1·32	 246 6·8 2·05 1·71 1·36
Results obtained with 0″·3 lengths of the wires, fixed between brass standards, with 10 cells of Silvertown Leclanché firing battery. **Note.** The resistance of the battery "leads" corresponds to 0·05 Ohm, which has therefore to be added to the resistance in all cases.	1 fused through 18 Ohms 2 „ „ 9 „ 4 „ „ 4·8 „ 12 „ „ 0·9 „ 24 bright red on short circuit (leads=0·05 Ohm)	1 fused through 24 Ohms 2 „ „ 11 „ 4 „ „ 6·5 „ 12 „ „ 1·8 „ 24 „ short circuit Battery leads=0·05 Ohm	1 fused through 27 Ohms 2 „ „ 15 „ 4 „ „ 8 „ 12 „ „ 1·7 „ 24 „ „ 0·3 „	1 fused through 15 Ohms 2 „ „ 7·45 „ 4 „ „ 3·7 „ 12 „ „ 0·3 „ 14 „ „ short [circuit (0·05) 24 dull red through short [circuit (0·05)	1 fused through 21 Ohms 2 „ „ 10·75 „ 4 „ „ 5·15 „ 12 „ „ 1·4 „ 24 bright red on short [circuit (0·05)	1 fused through 22 Ohms 2 „ „ 9·5 „ 4 „ „ 5·0 „ 12 „ „ „ 24 fused } on short „ bright red } circuit (0·05) The battery had been reduced somewhat in power by previous experiments before these trials.
Results obtained under the same conditions as above, with 5 cells of a large experimental Leclanché battery, specially constructed for firing guns simultaneously in divided circuit. **Note.** The resistance of the battery "leads" corresponds to 0·05 Ohm, which has therefore to be added on to the resistance in all cases.	1 fused through 8·6 Ohms 2 „ „ 4·65 „ 4 „ „ 2·95 „ 8 „ „ 1·45 „ 12 „ „ 1·30 „ 15 „ „ 0·8 „ 24 „ „ 0·5 „ 24 just fired gun cotton [through 1·9 Ohm	1 fused through 12 Ohms 2 „ „ 7·95 „ 4 „ „ 4 „ 8 „ „ 2·35 „ 12 „ „ 1·8 „ 24 „ „ 1·7 „ 24 just fired gun cotton [through 2·4 Ohms No. 1 cell of battery heated 24 to bright redness on short circuit (0·05 Ohm). The wires appeared to be approaching fusion, but 20 wires afterwards tried were not fused.	1 fused through 13 Ohms 2 „ „ 7·65 „ 4 „ „ 3·9 „ 8 „ „ 2·4 „ 12 „ „ 2·0 „ 24 „ „ 1·0 „ 24 just fired gun cotton [through 2·6 Ohms	1 fused through 7·2 Ohms 2 „ „ 4·25 „ 4 „ „ 2·2 „ 8 „ „ 1·3 „ 12 „ „ 0·89 „ 24 „ „ 0·37 „ 24 just fired gun cotton [through 1·3 No. 1 cell of the battery heated 24 to full redness on short circuit (0·05 Ohm)	1 fused through 11 Ohms 2 „ „ 5·50 „ 4 „ „ 3·25 „ 8 „ „ 2·0 „ 12 „ „ 1·5 „ 24 „ „ { 0·5 / 0·5 24 just fired gun cotton [through 2·0 Ohms	1 fused through 11 Ohms 2 „ „ 5·65 „ 4 „ „ 3·15 „ 8 „ „ 1·9 „ 12 „ „ 1·3 „ 24 „ „ { 0·6 „ / 0·7 „ 24 just fired gun cotton [through 1·9 Ohm

TABLE II.

Description of Wire.	Results of Exposure in Air.	Results of Contact with Air-dry Powder.	Results of Contact with Air-dry Gun-cotton.	Results of Immersion in Wet Gunpowder.
	Original R. of piece tested = 61·6 Ohms.	Original R. of piece tested = 62·6 Ohms.	Original R. of piece tested = 55·1 Ohms.	Original R. of wire tested = 15 Ohms.
	After 3 days, R. = 61·7 „	After 3 days, R. = 63 „	After 3 days, R. 55·2 „	After 2 days, R. = 22·3 „
	„ 4 „ 61·8 „	„ 4 „ 63 „	„ 4 „ 55·3 „	„ 4 „ 29·9 „
	„ „ 62·2 „	„ 6 „ 63·4 „	„ 6 „ 56·0 „	„ 6 „ continuity broken by corrosion
	„ 8 „ 61·8 „	„ 8 „ 63·3 „	„ 8 „ 55·4 „	Results of immersion in Wet Saltpetre.
German Silver	„ 12 „ 61·8 „	„ 12 „ 63·3 „	„ 12 „ 55·4 „	Original R. = 31·90 Ohms.
0″·0012 diameter	„ 15 „ 61·8 „	„ 15 „ 63·4 „	„ 15 „ 55·6 „	After 3 days = 32·1 „
	„ 20 „ 62·0 „	„ 20 „ 63·4 „	„ 20 „ 55·7 „	„ 12 „ 32·3 „
	„ 32 „ 62·1 „	„ 32 „ 63·5 „	„ 32 „ 55·9 „	„ 22 „ 32·3 „
	„ 40 „ 62·1 „	„ 40 „ 63·6 „	„ 40 „ 56·0 „	
	„ 50 „ 62·1 „	„ 50 „ 63·6 „	„ 50 „ 56·0 „	
				Original R. of wire tested = 13·8 Ohms.
	Original R. of piece tested = 34·1 Ohms.	Original R. of wire tested = 33·6 Ohms.	Original R. of wire tested = 33· Ohms.	After 2 days in wet powder 13·8 „
	After 2 days, R. = 34·2 „	After 2 days, R. = 33·6 „	After 2 days, R. = 33 „	„ 4 „ 13·8 „
	„ 6 „ 34·2 „	„ 6 „ 33·6 „	„ 6 „ 33 „	„ 6 „ 13·8 „
Platinum-Silver	„ 9 „ 34·2 „	„ 9 „ 33·6 „	„ 9 „ 33 „	„ 8 „ 13·8 „
0″·0018 diameter	„ 15 „ 34·2 „	„ 15 „ 33·6 „	„ 15 „ 33 „	„ 12 „ 13·9 „
	„ 28 „ 34·2 „	„ 28 „ 33·6 „	„ 28 „ 33 „	„ 18 „ 13·8 „
	„ 36 „ 34·15 „	„ 36 „ 33·6 „	„ 36 „ 33 „	„ 28 „ 13·8 „
	„ 46 „ 24·2 „	„ 36 „ 33·6 „	„ 46 „ 33 „	„ *38 „ 13·8

in promoting corrosion in its capacity as an electrolyte is interestingly demonstrated by the fact that the German silver wire remained buried for twenty-two days in wet saltpetre without undergoing great oxidation (which oxidation did not progress after that time), while the wire which was buried in wet gunpowder was destroyed by corrosion within six days.

The foregoing comparative experiments appear to warrant the conclusion that a wire consisting of the alloy composed of about 66 parts of silver and 33 of platinum is not only superior to a platinum wire of considerably smaller diameter as regards the resistance it offers to the passage of an electric current, but that it is also, practically, quite equal in this respect to a German silver wire of the same diameter, and is at the same time greatly superior to the latter in its power to resist corrosion.

In selecting the platinum-silver alloy as the material for the reproduction of standards of electrical resistance, Matthiessen referred (in his Report to the British Association Committee) to certain other advantages which it possesses, besides its low conducting power and its comparative freedom from liability to oxidation, namely, the slight extent to which its electrical resistance is altered by changes of temperature and its unalterability in this respect by exposure for several days to 100° C. These properties, though valuable with respect to its application as a trustworthy material for furnishing delicate measurements of electrical resistances, do not practically enhance its value as a material for the construction of fuses. German silver ranks very little below it as regards the slight extent to which its conductivity is affected by changes of temperature (the increment in the resistance of the latter, due to elevation in temperature from 0° to 100°, being 4·4 per cent., and that of the platinum silver alloy 3·2 per cent.). The alloy which, among many experimented with by Matthiessen, was most affected in its conductivity by changes of temperature sustained an increment of resistance of 28 per cent. in passing from 0° to 100°; but the slight alteration in resistance which the short piece of wire (0″·25 or 0″·3) in a fuse made even with this alloy could sustain from extreme fluctuations of *atmospheric* temperature would be of

no practical importance, being far exceeded by the accidental variations in the resistances of fuses made with fine wire, which are unavoidable in their manufacture, as will be presently shown. The platinum silver wires differ, however, in one respect very decisively from the other wires at present referred to as a means of electrical ignition, namely, in being much more readily fusible; and it appears not improbable that this peculiarity gives to wires of this alloy a decided advantage over those of platinum, and of an alloy of that metal to be presently mentioned, in their special application to the simultaneous explosion of a large number of fuses, arranged in *divided* circuits of different lengths, in consequence of the certainty of an instantaneous interruption of each circuit by the fusion of the wire.

Mr. G. Matthey has pointed out to me a practical difficulty in the preparation of this particular platinum-silver alloy, which, unless especial attention be directed to it, may constitute a serious objection to the employment of the alloy in question in the construction of fuses which should combine high resistance with uniformity. This arises out of a tendency to the separation of the metals (which are of widely-different specific gravity) from each other to a very considerable extent, unless strict attention be paid to the temperature at which the alloy is cast, as well as to the cooling of the casting. Matthiessen's experiments with different alloys of silver and platinum appear to have shown that, down to a certain point, the conductivity of the alloy increased approximately in direct proportion to the decrease of the proportion of platinum; thus, the conductivity of the 33 per cent. alloy being = 6·7, that of an alloy containing about 10 per cent. of platinum was = 18; with only 5 per cent. of platinum the conductivity was, however, proportionately very much greater (= 31·64). It is evident from the statements of Mr. Matthey, whose experience in the manufacture of alloys of platinum is unrivalled, that special attention and experience are required to insure the production of an uniform platinum-silver alloy, with the high proportion of platinum essential to the preparation of a wire possessing the low conductivity which will enable it to compete with German silver.

It is hardly necessary to state that the metal *iridium*, which is associated in nature with platinum, and is, indeed, contained in very appreciable quantities in most specimens of even carefully purified platinum, is readily alloyed with it; and the fact that iridium reduces the conductivity of platinum very greatly was demonstrated by Matthiessen's experiments. An alloy of platinum with 33·4 per cent. of iridium was found by him to have a conductivity of 4·54; that of the 33 per cent. platinum-silver alloy being 6·7. These circumstances led me to make some experiments with wires of iridio-platinum. In the first instance, Messrs. Johnson and Matthey furnished me with a wire stated to contain 7 per cent. of iridium, its diameter being 0″·0015 (=0·375 grain per yard). The resistance of this wire was found to be = 302 Ohms. per yard (that of 0″·25 of the wire being 2·22 Ohms.); it therefore exceeded that of the *finest* platinum silver (0″·001 diameter) experimented with. An iridio-platinum wire of the latter diameter was next drawn for me by Johnson and Matthey, but its resistance did not prove to be nearly as much greater as was naturally anticipated from a reduction by one-third of the diameter of the wire. On inquiry I learned that the two wires were not produced from one and the same parcel of alloy, and that, although this was stated to contain 7 per cent. of iridium, the latter metal employed in its production varied considerably in quality (containing probably osmium and other impurities), as the purposes for which the alloy was prepared did not necessitate a recourse to metals approaching purity.

Mr. G. Matthey afterwards produced for me two alloys of platinum with carefully prepared iridium, the one containing 7, the other 10 per cent. of this metal. Their resistances were found to be as follows:—

Nature of alloy.	Diameter of Wire.	Resistance per yard.	Resistance calculated for a wire 0″·001 diameter.
7 per cent. iridium	0″·022	0·841 Ohm.	224·4 Ohms.
10 per cent. iridium	0″·022	0·999 Ohm.	265 Ohms.

These wires were therefore proportionately of considerably lower resistance than the iridio-platinum wire first tried, but that of the 10 per cent. alloy was still sufficiently high to render a further

examination of it, in comparison with the platinum-silver alloy, desirable. A wire, of the smallest diameter used in these experiments, was therefore drawn of it, with the following results :—

Diameter.	Weight per yard.	Resistance per yard.
0″·001	0·27 grain.	273 Ohms.

Therefore, 0·25 inch of this wire (the length conveniently used in a wire fuse) would offer a resistance of 1·9 Ohms., which is but very little below that of a platinum-silver wire of the same diameter (2·05 Ohms). This iridio-platinum wire will consequently prove as efficient in the production of sensitive fuses as the finest platinum silver wire; it possesses, moreover, the advantage of much greater strength, and it will certainly be more reliable than wire of that material as regards uniformity of composition. It is, however, much less fusible than the platinum-silver wire, as the fusibility of platinum is reduced by being alloyed with iridium; this may, possibly, give the platinum-silver wire an advantage in the case of "fork-firing," with a large number of branch circuits of different lengths (as in arrangements for the simultaneous discharge of a number of guns).*

It is evident that wire fuses of considerably higher resistance than any referred to in this paper may be produced by employing a wire containing a higher proportion of iridium than 10 per cent. Mr. Matthey has kindly offered to furnish me with wires produced from a series of carefully-prepared iridium alloys which he intends producing, and which will therefore most probably afford an excellent opportunity for ascertaining the merits of wire-fuses of comparatively very high resistance. It appears probable, from the results obtained with the carefully-prepared alloys, containing only 7 and 10 per cent. of iridium, that the conductivity of a 33 per cent. alloy is considerably below that at which it was fixed by Matthiessen.

* Some practical experiments, made since this paper was read, have demonstrated that an iridio-platinum wire of the smallest diameter, and containing about 10 per cent. of iridium, is quite as efficient as the platinum-silver wire of corresponding diameter when applied in the above direction.

One other alloy has been specially recommended to me for experiment by Mr. Matthey, as being much more readily produced of uniform composition than the platinum-silver alloy, and as being a very readily workable material. Matthiessen experimented with an alloy of silver with 25 per cent. of palladium, and found it to rank next above the platinum-silver alloy in conducting power. The alloy suggested for trial by Mr. Matthey contains 40 per cent. of palladium and 60 per cent. of silver. A fine wire prepared of this material furnished the following result:—

Diameter of wire.	Weight per yard.	Resistance per yard.
0″·001	0·142 grain.	244·6 Ohms.

Therefore, 0″·25 of this wire would offer a resistance of 1·7 Ohms., which is about the same as that of the thicker of the two platinum-silver wires experimented with. It is still more fusible than the latter, and ranks at least on an equality with it as regards power of resisting corrosion, so far as severe test-experiments have been carried up to the present time. There is little doubt, therefore, that this alloy of palladium is also a very efficient material for the construction of wire fuses, offering considerable resistance.

In concluding these notes it may be useful to offer a few remarks relating to the construction of low-tension or wire fuses. It need hardly be stated that uniformity in resistance is an important quality to be aimed at; it is, moreover, a quality the attainment of which does not solely depend upon the employment of a wire of uniform conductivity, but also demands careful attention to details in the construction of the wire bridge and in the application of the igniting material or "priming" round the bridge, the necessity for such care increasing with the fineness of the wire employed in the fuse.

Uniformity in the resistance of wire fuses made with one particular kind and size of wire depends obviously in the first instance upon the employment of the same length of wire as the bridge in each fuse. With a view to attain this end the copper terminals must be accurately fixed at the same distance apart in the several fuses: the wire must be always uniformly stretched across between these

terminals (and therefore strained from one to the other as tightly as possible), and its two extremities must be attached to the terminals as uniformly as possible. The following general directions for constructing the terminals and wire-bridge will indicate the manner in which uniformity in the resistance of the fuses may be attained.

The bared ends of the insulated copper wires which are to form the terminals are firmly fixed at a uniform distance apart (0·25 inch is found a very convenient distance) by being inserted into a die or mould, and partly imbedded, while in that position, in a plug of some very hard cement or composition, which attaches itself thoroughly to the copper surfaces. A mixture of plaster of Paris or Portland cement and sulphur, which is cast round the wires while sufficiently hot to be viscid, is a good material for the purpose; or the wires may be imbedded in a plug of india-rubber preparation of the proper composition for subsequent conversion into ebonite. In either case the sulphur, which is a component of both preparations, attacks the surfaces of the copper wires, and thus insures their being quite immoveable when the plug has cooled. The bare ends, or terminals, should project about 0·25 inch beyond the plug in which the wires have been fixed, and should be uniform in length and well-brightened. In stretching the fine wire across from one terminal to the other, pains should be taken to do this always as tightly as possible, and in a perfectly horizontal line, so as to insure the employment of uniform lengths of wire in different fuses. Means should be adopted for making the terminals hold the wire, by the obvious expedients of roughening the surfaces of the copper, or of cutting a fine slit into their extremities, into which the wire may be inserted. The latter should, when properly strained and wrapped round, or let into the terminals, be firmly attached to these with solder, carefully applied by means of a small rod to *the back* of the terminals, so as not to be brought into contact with the wire-bridge itself.

By following simple directions such as the above, fuses of very fairly uniform resistance can be readily made, but the difficulties of manipulation are obviously somewhat increased when very fine

wires such as those spoken of in the foregoing are employed. With wires such as the finest of German silver, iridio-platinum, and platinum-silver, of which a bridge of 0″·25 offers a resistance of 2 or 2·3 Ohms., fuses may with a little practice be made of which the resistances range from 2 to 2·8 Ohms., that of the larger proportion being within 0·4 Ohm. in excess of the theoretical resistance. Until lately but little attention has been directed to the adoption of simple means for avoiding irregularities in resistance due to imperfect manufacture. Thus, in one well-known form of fuse, made with a comparatively coarse platinum wire (0″·003 in diameter), no special precaution has been taken to insure the wire being at all tightly stretched across the terminals, hence the resistance of a large proportion of these very greatly exceeds the theoretical number. Occasionally, in applying solder to the wire bound round the terminals, a small portion has been allowed to run on to the wire forming the bridge, and in such cases the resistance of the fuses has been importantly diminished. In another form of wire fuse (of German manufacture) the connections between the terminals and the wire bridge are very imperfect, and one pole of the fuse consists of a considerable quantity of fine copper wire which was found alone to offer a resistance of about 0·25 Ohm., thus considerably increasing the work to be done, without any useful effect, by the current employed.

Either gun-cotton or gun-paper is now very generally employed as a *priming* agent in platinum wire fuses, *i.e.* as a means of facilitating the production of explosion by such fuses, because the heat developed on passing a current through a fuse, if it should be insufficient to melt the wire, or to raise it to the temperature necessary for the ignition of gunpowder, may yet suffice to ignite the comparatively readily explosive gun-cotton. A small tuft of gun-cotton wool is generally pressed round the wire bridge, or a piece of gun-cotton yarn is tied upon it; but, though either of these modes of applying the gun-cotton priming can be readily used in fuses containing the comparatively coarse and strong wire hitherto employed, they are not so easily applicable in conjunction with fine wires, as these are liable to be broken in the operation, or as, for

fear of this occurring, the priming is not brought into properly close contact with the whole of the bridge, a condition which increases in importance as the source of heat in the fuse is diminished by a reduction of the thickness of the wire.* A simple modification in the mode of applying the gun-cotton insures a thoroughly efficient priming of the fuse, without any risk of the fracture of the bridge being incurred. A very fine gun-cotton powder or dust is prepared by taking dry pulped gun-cotton, or compressed gun-cotton, scraped or broken up to powder, and sifting this through muslin; the dust thus obtained is intimately mixed by means of a feather or hair-pencil with sufficient mealed gunpowder or detonating powder to make it flow readily into a small cavity. When the bridge of the fuse has been fixed in position into the fuse-head or case, this priming powder may be poured in and made to surround the bridge thoroughly and compactly by a gentle tapping or shaking of the case, without the slightest risk of breaking the bridge.

The foregoing few general instructions are of a nature almost to suggest themselves to those who set about the construction of low-tension fuses with the desire to insure great sensitiveness and uniformity. There is no doubt that, by combining care in construction of the fuse with the employment of fine wires of the nature referred to in these notes, the low-tension or wire fuse attains a degree of sensitiveness and uniformity which must render it a formidable and in many instances a very successful rival of the high-tension fuse in directions in which hitherto the latter has been pre-eminent for efficiency, more especially in connection with the employment of simple, powerful, and portable electrical appliances for the explosion of mines.

* Several instances have occurred, in the course of my experiments with fuses containing very fine wire of platinum-silver surrounded by gun-cotton powder, of the wire being fused by the passage of the current, without igniting the gun-cotton in which it was loosely imbedded.

The following Candidates were balloted for and declared duly elected:—

FOREIGN MEMBER:

Luigi Cappanera . . .	Florence.

MEMBERS:—

Robert V. Dodwell . .	Leadenhall Street.
William Hooper . . .	Beechwood, Clapham.
Captain McEvoy, C.E. .	London Ordnance Works.

ASSOCIATES:—

P. F. Barry	St. Vincent.
James Carpenter . . .	Paddington.
Paul Estler	Charlton.
J. Howe	Pernambuco.
Captain J. Hunter, R.N. .	United Service Club.
Thomas Miller . . .	Worcester
William Owen . . .	Monmouth.
C. H. Reynell . . .	Madeira.
Thomas Stout . . .	Lerwick.
W. B. Walpole . . .	Brighton.
Major A. Wood . . .	Abbey Wood.

The Meeting then adjourned.

ORIGINAL COMMUNICATIONS.

12, Queen Anne's Gate, Westminster, S.W.,
17th December, 1874.

The Secretary *of the* Society *of* Telegraph Engineers.

Dear Sir,—The publication of the enclosed Paper, which formed the subject of the Bakerian Lecture for the year 1871, has been delayed beyond reasonable limits, owing to its incompleteness in many respects at the time of its delivery through the pressure of professional duties, which both then and since has rendered it almost impossible for me to snatch, even occasionally, a few hours' leisure for the pursuit of purely scientific objects. Even now the inquiry into the dependency of electrical resistance upon temperature at the higher temperatures is not nearly as complete as I should have desired. The measuring instruments described still leave ample scope for improvement; they have, however, already done useful work in the hands of scientific inquirers into the effects of heat, and this remark particularly applies to the Electrical Pyrometer.

All the information on the subject of these instruments which is now before the public is that contained in the Proceedings of the Royal Society, in the publications of the Royal Institute, and in a Paper by Professor Weinhold contained in the Programme of the Royal Technical School at Chemnitz.

Under these circumstances, and in view of the fact that these publications do not give any account of the large series of experiments which I undertook in order to satisfy myself of the accuracy of this method of measuring temperature, and which have been supplemented at intervals during the last three years, I have decided on giving the information in its present form in order that the scientific public may have the means of knowing the experimental tests upon which the instruments have been based.

I am, dear Sir,

Yours faithfully,

C. WILLIAM SIEMENS.

ON THE

DEPENDENCE OF ELECTRICAL RESISTANCE ON TEMPERATURE.

PART FIRST.

ON THE INFLUENCE OF TEMPERATURE UPON THE ELECTRICAL RESISTANCE OF METALLIC CONDUCTORS.

THE experimental researches hitherto published on this subject have been limited to temperatures ranging from the freezing to the boiling point of water, and great uncertainty still prevails regarding the law of increase at temperatures exceeding 100° Cent.

The early experiments made by Arndsten * and Dr. Werner Siemens † tend to show that copper, silver, and other pure metals offer electrical resistances which increase with the temperature in an arithmetical ratio within the limits of their experiments, which extended from 0° to 100° Centigrade, whilst subsequent researches by Dr. Matthiessen indicate a slightly divergent ratio between the same limits of temperature.

Platinum, which is, in many respects, a suitable metal for extending these enquiries to higher temperatures, has been left out of consideration in the otherwise exhaustive researches of Matthiessen, and when I first directed my attention to this metal, I observed very extraordinary differences in the electrical conduction of different specimens.

Platinum wire.

I found it impossible to obtain platinum wire of such a degree of purity that its co-efficient of increment should have a value corresponding with that of silver, and the other pure metals. Some platinum wire, drawn for me by Messrs. Johnson and Matthey some years since, gave, when measured, a conducting power only 4·7 times that of mercury. Its increase of resistance was from

* *Vide* "Annal. de Chimie," vol. liv. 1858, p. 440–443.

† *Vide* Poggendorff's "Annalen," vol. cx. p. 1, vol. cxii. p. 353.

0·95 units at 20° C. to 1·12 units, at 100° C.; or 22·4 per cent. This platinum had been prepared by fusion in a De Ville furnace. Platinum recently supplied to me by the same firm, prepared by the old method of forging, had a conducting power of 8·2, whilst it increased in resistance from 0·97 units at 20° C. to 1·23 units, at 100° C., or 33·5 per cent. This led me to believe that the process by which platinum is prepared has much to do with its behaviour as a conductor, owing probably to a slight admixture of iridium and other metals of that class, in the fused metal; a supposition which is sufficiently proved by the results tabulated below, and from which it follows, that great caution is necessary in selecting platinum-wire for electrical experiments; and that the fusion of a wire of a given length and diameter for instance, is by no means a test of the strength of an electrical current.

	Kind of Platinum.	Diameter (inches).	Length (inches).	Resistance at 73° Fahr.	Conducting power at 73° Fahr.
1	Pure melted, No. 1	0·062	507·5	0·790	8·6
2	Common soft	0·062	580·5	0·985	7·9
3	Platinum with 5 per cent. iridium	0·021	112·5	1·800	7·2
4	Pure melted, No. 2	0·021	195·0	2·805	8·16
5	Pure forged	0·021	292·0	4·000	8·85
6	Impure melted	0·021	—	—	4·7

The percentage increment of increasing resistance of all these specimens was lower than that of pure silver or copper; but this is really of little practical importance in view of the second part of this inquiry, provided that its coefficient is known, and that it remains constant. A higher coefficient would be of advantage only in so far as by giving greater differences of resistance for given differences of temperature, the readings with it would be proportionately more delicate.

In carrying out my experimental inquiry regarding the dependence of electrical resistance upon temperature, I employed platinum wire of ·009 inches diameter, which had been prepared,

by the old welding process (which gives, as already stated, a much more conductive, and, therefore, a purer wire than the more recent process by fusion in a De Ville's furnace). In one of the series of experiments, this wire was wound upon a cylinder of pipeclay, in helical grooves to prevent contact between the convolutions of the wire. To arrive at a knowledge of its electrical resistance, when subjected to various temperatures, I placed it, together with a delicate mercury thermometer made for me by Messrs. Negretti and Zambra, in a copper vessel, contained in a bath of linseed oil, which (in order to prevent the too sudden radiation of its heat, and consequent variation of temperature) was placed within a larger vessel, the space between the two being packed with sand. The leading wires of the platinum-coil were then connected with a Wheatstone's balance and a delicate galvanometer. The bath was very gradually heated by a series of small Bunsen's burners, and whilst the oil was kept in continual motion, the resistance of the platinum wire was read off at intervals of 4° or 5° Centigrade. When the highest point had been reached, the bath was allowed to cool down gradually, and measurements were taken at the same points of temperature as before. This was repeated several times, until about six readings of the resistance of the wire at each point of temperature had been obtained. The mean readings are contained in the first table given at the end of this Part. The platinum wire was carefully annealed, and maintained for several hours at the maximum heat before the observations were taken.

Not satisfied with this single series of experiments, I undertook a second series under somewhat different conditions. Instead of coiling the wire upon a pipe-clay cylinder, I employed a spiral contained in a glass tube and hung by its leading wires in a rectangular air-chamber, about 6 inches long, 3 inches broad, and 3 inches deep, the space between the walls being filled with sand to insure a very steady temperature inside. Three mercury thermometers were inserted through the cover of this double chamber, so that their bulbs stood around the platinum coil in the same horizontal plane. This box was heated externally, by five small Bunsen's burners, a gas pressure regulator being applied to give steadiness of heat. Irregular losses of heat by radiation, or by atmospheric currents, were prevented by a metallic screen surrounding the flames and the heated box.

This apparatus is represented in the accompanying figure.

Fig. 1.

The temperature of the box was gradually raised to 350° Centigrade, and then lowered; and observations were taken at regular intervals of increasing and decreasing temperature.

The results obtained in this further set of experiments are given in the second table. The wire employed was not the same as that employed in the first series, which accounts for certain differences in the ratio of increase observed, although in other respects the accordance of the two series may be considered satisfactory.

In order to test these discrepancies, a third set of experiments was undertaken, with the same platinum wire which had been employed in the second set, with the difference, that the chamber containing the tube and wire and the thermometers was filled with linseed oil. The results are given in the third table, in two brief series, the object being, in this case, to test the former experiments by a few very careful observations in which the flames were so adjusted by a gas-pressure regulator, that a perfectly steady heat could be maintained for an hour, or more, to insure identity of temperature in every part of the chamber.

The general accordance between these results is best shown in the accompanying diagram No. 1, where the first, second, and third series of observed results are represented by the lines marked 1, 2 and 3, respectively. The horizontal divisions of the sheet represent Centigrade degrees of temperature measured from the absolute zero of temperature; and the vertical divisions units of resistance divided into tenths.

With the exception of one observation, which has evidently

been taken or noted in error, the accordance between the second and third series of observations is satisfactory. They represent a line, curved downwards towards the X axis, which it crosses at a point near the absolute zero, or 274° Centigrade below the freezing point of water.

Copper, iron, silver, aluminium.

No general conclusion could, however, be drawn from the bearing of one metal. I procured, therefore, wires of comparatively pure copper, of fused iron (or mild steel), of silver, and of aluminium, which were subjected to the same series of observations, as before described. The results are given in tables 4 to 7, and are also laid down on the diagram according to the same scale as the platinum curve.

Setting aside some palpable errors, these results also produce lines curving downwards to the absolute zero on the abscissal axis, and agree very closely with the measured results obtained by Dr. Matthiessen* between the limits of 0° and 100° Centigrade. They also agree, generally, with the results I obtained by means of another series of observations which I undertook for testing the progressive increase of resistance beyond the range of the mercury thermometer, and which will be noted further on.

Encouraged by these concordant results, I have endeavoured to find a general expression for the increase of electrical resistance in conductors with rise of temperature, which should be based upon a rational dynamic principle.

The experimental curves represented on the diagram differ so little from a straight line, between the limits of 0° and 100° Cent., that the early observers, whose observations did not go beyond those limits, naturally concluded that the electrical resistance increased in an arithmetical ratio with the temperature. In taking the amount of increase between these limits, in copper or silver wire, it was, moreover, found to coincide very nearly with the increase of volume of permanent gases by heat.

Clausius has drawn from these data the conclusion "That the resistances of metals are directly proportional to their absolute temperatures."† Matthiessen, however, found that the increment of increase of resistance was not absolutely constant between

* *Vide* "Philosophical Transactions," 1862.

† *Vide* Poggendorff's "Annalen," Vol. civ., p. 650, 1858.

the limits of 0° and 100° Cent., but that the ratio of increase in pure metal was expressed by the formula

$$R^t = \frac{R^o}{1 - \cdot 0037647\,t + 0 \cdot 00000834\,t^2},$$

where R^o represents the resistance at zero Centigrade and R^t at any other temperature on the same scale, which ratio agrees very closely with my own results between those limits; whereas, at temperatures exceeding 100°, great discrepancies are at once apparent. This will be seen from the following statement of calculated resistances for the higher temperatures by Matthiessen's formula—

Temperature in degrees Cent.	Resistance in Units.
t = 0°	Rt = 1·0000
= 100°	= 1·4146
= 300°	= 1·6098
= 600°	= 0·8314
= 1000°	= 0·1794
= 2000°	= 0·0373

His formula is indeed inapplicable to temperatures exceeding 100° Cent. He adds, it is true, a fourth member to his denominator, containing t^3, which has the effect of harmonizing it more completely with the observed values at low temperatures, without, however, producing more reasonable values for high temperatures. This formula, then, is applicable only within the narrow range of the experiments by which it was determined.

Law of increased resistance. Now if we apply the mechanical laws of work and velocity to the vibratory motions of a body which represent its free heat, we should define this heat as directly proportional to the square of the velocity with which the atoms vibrate. We may further assume that the resistance which a metallic body offers to the passage of an electric impulse from atom to atom is directly proportional to the velocity of the vibrations which represent its heat. In combining these two assumptions, it follows that the resistance of a metallic body increases in the direct ratio of the square root of the free heat communicated to it.

Algebraically, if (r) represent the resistance of a metallic conductor at the temperature T, reckoning from the absolute zero, and

α an experimental co-efficient of increase peculiar to the particular metal under consideration, we should have the expression

$$r = \alpha T^{\frac{1}{2}}.$$

This purely parabolical expression would make no allowance for the probable increase of resistance, due to the increasing distance between adjoining particles with increase of heat, which would depend upon the co-efficient of expansion, and may be expressed by βT, which would have to be added to the former expression. To these factors a third would have to be added, expressing an ultimate constant resistance of the material itself at the absolute zero, and which I call γ. The total resistance of a conductor at any temperature, T, would, therefore, be expressed by the formula

$$r = \alpha T^{\frac{1}{2}} + \beta T + \gamma \quad (1).$$

The law of increase expressed by this formula is graphically represented by diagram No. 3, the spaces between the abscissal axis and the parabola expressing the resistances due to the absolute motion of the particles; the arithmetically increasing field of resistance above the parabolic curve expressing the increase due to increase of distance between adjoining particles; and the field below the X axis, the constant resistance of the material under all conditions. It remained to be seen whether, in giving suitable values to the co-efficients α, β, and γ, this law of increase could be made to coincide with the observed results.

In deciding the abscissal axis according to temperature, and fixing the zero Centigrade at the point where the ordinate equals a unit of resistance in the first diagram; or an amount equal to the specific resistance at that temperature in the second, it will be observed that the portion of the curve where the absolute zero (or any other point of the thermal scale) falls, is completely fixed; and it was important to see whether, in starting from that point, the curvatures as represented by the above formula would agree with those of the experimental lines of the different metals.

Three points of each of the experimental curves, including the zero Centigrade, were taken, and the experimental values for T and r at these points being put into the above formula, the numerical values for α, β, and γ were obtained for each metal. If written down with these numerical co-efficients, the formula is as follows:

For platinum—$r = \cdot 0021448 T^{\frac{1}{2}} + \cdot 0024187 T + \cdot 30425$

$r = \cdot 039369 T^{\frac{1}{2}} + \cdot 00216407 T - \cdot 24127$

$r = \cdot 092183 T^{\frac{1}{2}} + \cdot 00007781 T - \cdot 50196$

For copper—$r = \cdot 026577 T^{\frac{1}{2}} + \cdot 0031443 T - \cdot 29751$

For iron—$r = \cdot 072545 T^{\frac{1}{2}} + \cdot 0038133 T - 1 \cdot 23971$

For aluminium—$r = \cdot 05951436 T^{\frac{1}{2}} + \cdot 00284603 T - \cdot 76492$

For silver—$r = \cdot 0060907 T^{\frac{1}{2}} + \cdot 0035538 T - \cdot 07456$

Curves constructed in accordance with these expressions are shown in the portions below 0° C. and above 350° C., of diagram 1, with a constant resistance of 1 unit for each metal at the zero Centigrade, and in diagram No. 2, with each metal represented by its own specific resistance at zero Centigrade, and the close coincidence of the calculated resistances with the experimental resistances as shown in the tables, excepting a certain number of evidently erroneous observations, proves the entire applicability of the law of increase expressed by the formula to various metals at temperatures between 0° and 350° Centigrade. It remained to be proved, however, whether the same law would apply to higher degrees of temperature.

Platinum ball pyrometer.

Fig. 2.

For this purpose I had recourse to a pyrometer, constructed upon the supposition that the specific heat of solids and liquids is the same at all temperatures. An instrument of this description was designed by me some years since, and is used by ironmasters in determining the temperature of their hot blast. It is represented at fig. 2, and consists of a cylindrical vessel of thin sheet copper capable of containing an imperial pint of water. The inner vessel is surrounded by two external vessels of thin metal plate, the narrow space between the first and second being filled with air; and the space between the second and third, or the outer vessel, with cow-hair or other non-conductor of heat. A delicate thermometer is fixed against the side of the innermost vessel, being protected from injury by a perforated plate. It is

provided with a sliding scale having divisions equal in breadth to the degrees on the thermometer, but each division counting as the equivalent of 50 degrees. A copper or platinum ball is provided, the weight of which is so adjusted that the heat capacity of 50 balls, is equal to that of an imperial pint of water at ordinary temperature. This is dropped into the vessel and the sliding scale thereupon fixed so that its zero index shall coincide with the position of the mercury level in the thermometer tube. The copper or platinum ball is perforated, in order that it may be placed at the end of a rod to be exposed to the heat which is intended to be measured.

Upon being fully heated, the ball is dropped into the water, and the reading indicated upon the sliding scale, added to that of the mercury thermometer, gives the temperature of the ball.

Although a high degree of accuracy cannot be claimed for this instrument, its indications are, nevertheless, useful for obtaining fixed ratio indications of the higher temperatures. It has enabled me to test the general accuracy of the ratio of increase of electrical resistance beyond the limits of the more correct tests obtained at the lower temperatures. The accuracy of these corroborative results depends upon the supposition that the specific heat of the metal ball is the same at high and low temperatures; but, although this may not be, strictly speaking, the case, there is evidence to show that the variations are not of serious import, except probably in nearing the melting points.

The following are some comparative results which have been obtained by placing in the same heated chamber a copper ball of known capacity of heat, and a coil of platinum wire wound in the spiral grooves of a porcelain cylinder and protected from injury by a cylindrical casing of platinum. Both the copper ball and the protected spiral wire were placed inside the heated chamber in a piece of wrought-iron tubing, to ensure more complete identity of temperature, when the resistance of the spiral was taken, and the copper ball dropped into the apparatus just described.

The following are some of the results :—

Observed temperature by copper ball pyrometer.	Observed resistance of coil when heated.	Resistance of the same coil at 0° C.	Temperature of coil according to formula r. = ·0021448 T ½ + ·0024187 T + 0·30425	Difference.
835 C.	30·5	10·56	811° C.	−24°
854 „	32·0	10·56	882° „	+28°
810 „	29·6	10·56	772° „	−38°

It remains to be proved whether the law of increase of electrical resistance, which I have here ventured to put forward, holds good for all conductors; and whether it may be trusted at temperatures approaching either the point of absolute zero or the melting point of the metal under consideration. The whole subject, indeed, requires further and fuller investigation than I could devote to it with the principal object of my investigation in view, which, having been the construction of a reliable instrument for measuring low and high temperatures by electrical resistance, I have followed up this branch of the enquiry only to such a point as to supply a tolerably reliable basis for such practical purposes.

FIRST TABLE.

Showing the Measured Increase of Resistances with the Increase of Temperature of a Coil of Platinum Wire of 0·009 *inches diameter in Oil.*

Mean temperature of three mercury thermometers in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0·0	1·0000	1·0000	—	by inference
37·8	1·0989	1·0985	−·0004	
43·3	1·1129	1·1141	+·0012	
48·9	1·1269	1·1243	−·0026	
54·4	1·1405	1·1362	−·0043	
60·0	1·1543	1·1457	−·0086	
65·6	1·1676	1·1578	−·0098	
71·1	1·1812	1·1678	−·0134	
76·7	1·1947	1·1792	− ·015	

FIRST TABLE.—*Continued.*

Mean temperature of three mercury thermometers in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
81·8	1·2068	1·1912	—·0156	
87·8	1·2209	1·2030	—·0179	
93·3	1·2338	1·2237	—·0101	
98·9	1·2469	1·2369	—·0100	
104·4	1·2695	1·2493	—·0202	
110·0	1·2723	1·2611	—·0112	
115·6	1·2851	1·2743	—·0108	
121·1	1·2974	1·2952	—·0022	
126·7	1·3099	1·2856	—·0243	
132·2	1·3222	1·3195	—·0027	
137·8	1·3344	1·3317	—·0027	
143·3	1·3465	1·3392	—·0073	
148·9	1·3587	1·3511	—·0076	
154·4	1·3707	1·3644	—·0063	
160·0	1·3826	1·3773	—·0053	
165·6	1·3945	1·3911	—·0034	
171·1	1·4061	1·4088	+·0027	
176·7	1·4179	1·4426	+·0247	
182·2	1·4249	1·4327	+·0078	
187·8	1·4420	1·4411	—·0009	
193·3	1·4524	1·4530	+·0006	
198·9	1·4685	1·4615	—·0070	
204·4	1·4760	1·4745	—·0015	
210·0	1·4864	1·4797	—·0067	
215·6	1·4977	1·4909	—·0068	
221·1	1·5087	1·5096	+·0009	
226·7	1·5198	1·5132	—·0066	
232·2	1·5307	1·5323	+·0016	
237·8	1·5363	1·5450	—·0087	
243·3	1·5526	1·5535	+·0009	
248·9	1·5634	1·5596	—·0038	
254·4	1·5741	1·5778	+·0037	
260·0	1·5849	1·5908	+·0059	
265·6	1·5935	1·6007	+·0048	
271·1	1·6060	1·6001	—·0059	
276·7	1·6167	1·6195	+·0028	
282·2	1·6210	1·6295	+·0085	
287·8	1·6375	1·6375	—	

SECOND TABLE.

Showing the Measured Increase of Resistances with the Increase of Temperature of a Coil of Platinum Wire of 0·009 inches diameter, in Air.

Mean temperature of three mercury thermometers, in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0·	1·0000	1·000	—	In snow and water completely surrounding the oil chamber.
8·5	1·0135	1·012	−·0010	
12·0	1·0296	1·029	−·0006	
14·5	1·0358	1·036	+·0002	
22·3	1·0541	1·048	−·0661	
33·8	1·0835	1·085	+·0015	
34·0	1·0840	1·085	+·0010	
46·2	1·1141	1·107	−·0071	
86·0	1·2123	1·213	+·0007	
100·0	1·2468	1·250	+·0032	In the steam of boiling water. Barometer at 30 in.
112·7	1·2771	1·287	+·0099	
123·2	1·3040	1·309	+·0050	
148·0	1·3486	1·357	+·0264	
159·0	1·3922	1·404	+·0118	
160·3	1·4002	1·404	+·0038	
171·3	1·4224	1·426	+·0036	
187·3	1·4618	1·485	+·0132	
193·5	1·4770	1·492	+·0150	
196·0	1·4832	1·494	+·0108	
197·7	1·4873	1·496	+·0087	
201·0	1·4955	1·499	+·0035	
206·0	1·5080	1·507	−·0010	
206·3	1·5085	1·511	+·0025	
212·3	1·5233	1·529	+·0057	
214·0	1·5275	1·532	+·0045	
214·7	1·5292	1·535	+·0058	
222·0	1·5461	1·559	+·0129	
231·0	1·5693	1·556	−·0033	
238·6	1·5877	1·588	+·0003	
247·0	1·5986	1·603	+·0044	
254·0	1·6258	1·669	+·0332	
264·6	1·6518	1·654	+·0022	
275·0	1·6774	1·698	+·0106	
282·0	1·6946	1·702	+·0074	
804·0	1·7486	1·741	−·0076	
823·0	1·7953	1·780	−·0153	
334·0	1·8220	1·801	−·0210	
340·0	1·8370	1·824	−·0130	

THIRD TABLE.

Showing the Measured Increase of Resistances with the Increase of Temperature of a Coil of Platinum Wire of 0·009 *inches diameter in Oil.*

First Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing.
15	1·0372	1·04	+ ·0028	
100	1·2454	1·25	+ ·0046	In boiling water.
176	1·4356	1·43	— ·0056	
198	1·4900	1·49	—	
234	1·5788	1·58	+ ·0012	
287	1·7095	1·71	+ ·0005	
312	1·7710	1·77	— ·0010	
340	1·8400	1·84	—	

Second Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing.
15	1·0372	1·04	+ ·0028	
100	1·2454	1·25	+ ·0046	In boiling water.
162	1·4001	1·40	— ·0001	
208	1·5147	1·52	+ ·0053	
239	1·5911	1·70	+ ·1089	Evidently an error of observation or notation.
303	1·7489	1·75	+ ·0011	
346	1·8547	1·85	— ·0047	

FOURTH TABLE.

Showing the Measured Increase of Resistance with the Increase of Temperature of a Coil of Copper Wire of 0·008 *inches diameter.*

FIRST SERIES.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing.
23	1·0905	1·09	— ·0005	
100	1·3876	1·38	— 0076	In boiling water.
166	1·6396	1·63	— ·0096	
168·2	1·6480	1·64	— ·0080	
210	1·8053	1·80	— ·0053	
215	1·8240	1·82	— ·0040	
276	2·0514	2·04	— ·0114	
280	2·0662	2·05	— ·0162	
315	2·1958	2·17	— ·0258	
322	2·2316	2·20	— ·0316	
342	2·2958	2·26	— ·0358	

Showing the Measured Increase of Resistance with the Increase of Temperature of a Coil of Copper Wire of 0·008 *inches diameter in Oil.*

SECOND SERIES.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing.
15	1·0591	1·06	+ ·0009	
100	1·3886	1·39	+ ·0014	In boiling water.
182	1·7000	1·70	—	
220	1·8427	1·85	+ ·0073	
255	1·9734	1·98	+ ·0066	
296	2·1256	2·13	+ ·0044	
327	2·2400	2·24	—	
346	2·3100	2·32	+ ·0100	

FIFTH TABLE.

Showing the Measured Increase of Resistance with the Increase of Temperature of a Coil of Iron Wire of 0·0086 *inches diameter.*

First Series.

Mean temperature of three mercury thermometers in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing.
15	1·0897	1·09	+·0003	
100	1·5857	1·57	—·0157	In boiling water.
134	1·7758	1·78	+·0042	
148	1·8540	1·86	+·0060	
216	2·2292	2·10	—·1291	
260	2·4676	2·47	+·0024	Evidently an error of observation or notation.
305	2·7085	2·71	+·0015	
347	2·9300	2·93	—	

Second Series.

Mean temperature of three mercury thermometers in degrees Cent.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·0000	1·00	—	In ice surrounding the casing
15	1·0897	1·09	+·0003	
100	1·5857	1·57	—·0157	In boiling water
140	1·8094	1·75	—·0594	
198	2·1300	2·13	—	
256	2·4461	2·45	+·0039	Evidently an error of observation or notation.
313	2·7457	2·75	+·0043	
347	2·9300	2·93	—	

SIXTH TABLE.

Showing the Measured Increase of Resistance with the Increase of Temperature of a Coil of Aluminium Wire of ·008 *inches diameter.*

First Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·	1·	—	In ice surrounding the casing.
14·17	1·06548	1·062	—·00348	
118·5	1·53118	1·530	—·00118	
144·25	1·64253	1·642	—·00053	
211·17	1·92675	1·920	—·00675	
241·37	2·05288	2·044	—·00888	
283·2	2·22569	2·221	— ·00469	
304·8	2·31413	2·313	—·00113	

Second Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
304·8	2·31413	2·313	—·00113	
263·17	2·1432	2·124	—·0192	
170·2	1·75358	1·709	—·04458	
137·77	1·61463	1·565	—·04963	
89·9	1·40602	1·356	—·05002	
24·73	1·11388	1·071	—·04288	

SEVENTH TABLE.

Showing the Measured Increase of Resistance with Increase of Temperature of a Coil of Silver Wire of ·008 inches diameter.

First Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
0	1·	1·	—	In ice surrounding the casing.
19·93	1·07443	1·074	—·00043	
66·57	1·24817	1·26	+·01183	
118·53	1·44109	1·45	+·00891	
150·63	1·55999	1·56	+·00001	
219·1	1·81307	1·81	—·00307	
262·5	1·97313	1·98	—·00687	
303·8	2·12524	2·13	—·00476	

Second Series.

Mean temperature of three mercury thermometers in degrees C.	Calculated resistance of coil.	Measured resistance of coil (reduced).	Difference.	Remarks.
303·8	2·12524	2·13	—·00476	
275·1	2·01956	2·02	—·00044	
235·1	1·8721	1·87	+·0021	
219·5	1·81454	1·81	+·00454	
164·5	1·61132	1·60	+·01132	
115·5	1·42988	1·41	+·01988	
19·3	1·07208	1·069	+·00308	

PART SECOND.

On Measuring Temperatures, including Furnace Temperatures, by Electrical Resistance.

In the early days of submarine telegraphs, it frequently happened that the insulated conductor, which had tested well at the cable works, proved faulty after the cable had been submerged, and, upon examining such faulty cable, the metallic conductor was found to have sunk through the gutta percha covering, an effect which could not be satisfactorily accounted for by accidental causes, such as may arise in joining wires during the process of manufacture; whereas the effect of heat of an intensity of at least 38° Centigrade, or of sufficient intensity to soften or melt the gutta percha covering of the cable, was generally traceable.

In 1860, when professionally engaged on behalf of Her Majesty's Government in superintending the examination of the electrical condition of the Malta and Alexandria Telegraph Cable, during its manufacture and submersion, it appeared to me that heat, as revealed by its disastrous effects, might be spontaneously generated within a large mass of cable, either when coiled up at the works or on board ship, owing to the influence of the moist hemp and iron wire composing its armature. In considering the means by which such rise of temperature within the mass might be observed, my attention was directed towards that property of metallic conductors of offering, in a rising temperature, an increasing resistance to an electrical current, to which attention has been drawn in the First Part of this Lecture.

Now, an instrument constructed on the principle of the increase of electrical resistance with rise of temperature, would possess the obvious advantage that the metallic conductor under observation might be at some distance from the observing instrument, and need not be disturbed for making observations. Accordingly, I prepared coils of copper wire insulated with silk, whose electrical resistance having been ascertained and adjusted, were enclosed in iron tubes, with the ends hermetically sealed, but allowing thick

insulated leading-wires to pass outward. These protected coils were placed at various points within the mass of cable as it was coiled in the ship's hold, the insulated leading-wires being taken into the testing cabin. These arrangements proved of great utility in saving this and subsequent cables from destruction; for, although the external layers of cable remained cool to the depth that mercury thermometers could be inserted, the coils placed in the interior of the large mass indicated a steady rise of temperature which had reached 98° Fahr. when the official test was made. A few degrees of additional rise of temperature must have destroyed the insulation of the cable, I therefore urged that cold water should be poured over it. This was not effected without strong opposition on the part of the incredulous; but when at last the water of the Thames, which was covered at the time with floating ice, was pumped over the cable, it issued therefrom at the temperature of 78° Fahr., thus proving the general correctness of the electrical indications previously observed.

It may be here remarked, that in consequence of this practical test, the Government consented to the construction within the ship's hold of water-tight iron tanks, and also to the cable being submerged in water during its passage from the works to its destination, precautions which have ever since been adopted in laying submarine cables.

Stimulated by these results, it occurred to me that an instrument of more general application might be constructed for measuring the temperature of inaccessible places; and, that on the same principle, a reliable pyrometer might be made, an instrument of great requisition in the useful arts for obviating the uncertain and contradictory statements regarding the temperature at which smelting and other operations are accomplished. Various practical difficulties were encountered in working out these problems, which have, however, been gradually lessened or overcome, and my labours have resulted in the production of several types of thermometrical and pyrometrical instruments.

When the temperature of an inaccessible place whose temperature has to be measured is not above the boiling point of water, the thermometer coil is variously constructed, according to the position in which it may have to be placed.

Thermometric resistance coil.

The simplest of these is shown in the accompanying sketch, and consists of a spiral of insulated wire wound upon a cylindrical piece of wood or metal enclosed in a cylindrical silver casing, the two extremities of the wire being soldered to thicker insulated wires, a third thicker wire being joined to one of the other two, the three forming a light cable. This instrument I use for measuring ordinary temperatures on land, and in this form the apparatus would, I conceive, be useful to the physiologist or the medical man for ascertaining the temperature of the human body under certain influences without disturbing it. The instrument is extremely sensitive, and temperatures may, with a good Wheatstone balance, be read off to within a tenth of a degree Fahrenheit.*

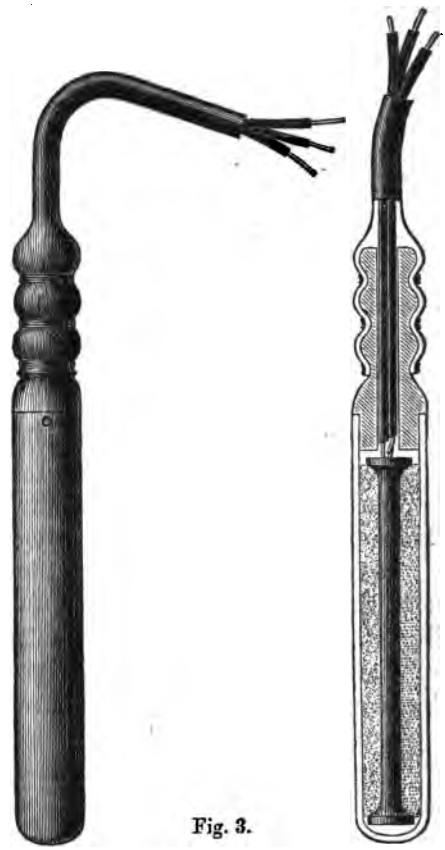


Fig. 3.

In this arrangement of apparatus the indications of the thermometric resistance coil, or instrument described, are read off by direct comparison with a mercury thermometer, which latter will represent the exact temperature of the former at a distance, it may be, of several miles.

* An instrument similar in arrangement to the one here mentioned was described by me before the Physical Section of the British Association at Manchester, in 1861; and a modified arrangement for measuring deep sea temperature was presented, in the joint names of Dr. Werner Siemens and myself, to the Berlin Academy in 1863.

The principle is as follows:—

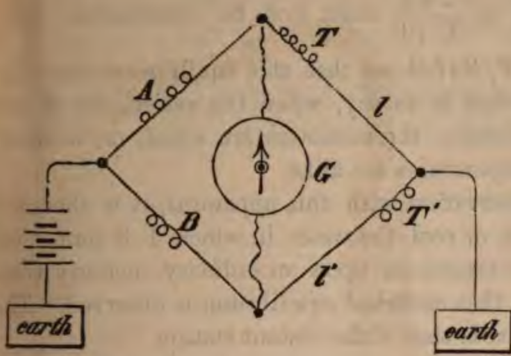


Fig. 4.

When two similar thermometer coils have different temperatures, they have also different resistances, and, therefore, in order to make them equal, the temperature of the one in the room must be made equal to that of the other at a distance. A plan of the way in which this is arranged is shown in fig. 4. The two resistances, A and B, forming the left-hand side of the parallelogram, consist of coils of silk-covered German silver wire, each of 500 units, and both wound upon the same bobbin, so as to have the same temperature. The resistance thermometer, T′, of about 500 units, is placed at the distant point, whilst the comparison thermometer, T, precisely equal in respect of material and resistance to T′, is placed in the testing room, and these are connected with the other resistances by the two leading wires, l and l′. The lower end of l is put to earth at T′, but the corresponding end of l is connected with one side of the resistance thermometer, T′, and then with the earth. In the testing room the leading wire, l′, is connected directly with the resistance, B, and with the galvanometer; whilst l is connected with the resistance A, the galvanometer and the balance thermometer, T. The leading wires, l and l′, are of copper, of the same gauge, insulated with gutta-percha and spun up together, so that they are equally affected by changes of temperature at intermediate places, and have therefore always equal resistances. For protection against mechanical injury, the leading wires are covered with hemp and sheathed with a laminated covering of copper.

Thus arranged, the balance thermometer, T, is immersed in a bath of water, the temperature of which can be varied.

When electrical equilibrium is to be obtained, it is evident that the relation $\frac{A}{B} = \frac{T+l}{T'+l'}$ must first be established. And since $A=B$ and $l=l'$, it follows that this equilibrium can only occur when $T=T'$; that is to say, when the resistances of the distant and of the balance thermometers are equal, or, in other words, when their temperatures are alike.

In making an observation with this apparatus, it is therefore only necessary to heat or cool the water in which T is immersed, and to read off its temperature upon an ordinary mercury thermometer the moment that electrical equilibrium is observed. The temperature thus noted is that of the distant station.

Thermometric comparison-coil. The comparison-coil, the temperature of which has to be adjusted, consists of a coil of fine silk-covered iron or copper wire, corresponding with the wire employed for, and of a resistance precisely equal to, that of the thermometer-coil at a standard temperature. It is wound upon a short length of metal tube and enclosed in an outer protecting capsule of silver, or other metal, to guard it against mechanical injury and against the ingress of water, which, by causing short circuits between the convolutions, would render its indications inexact. The open end of the protecting capsule is fitted with a vulcanite stopper through which two thick copper leading wires, forming the end of the resistance coil, are passed.

The water bath used with this instrument, and which I have found very convenient for raising or lowering the temperature of the comparison-coil to that of the distant spot, consists of a cylindrical copper vessel, on one side of which a mercury thermometer is fixed in a suitable frame; the bulb and lower part being protected by a perforated shield. There are two funnels for supplying hot and cold water respectively. The cold water pipe ends near the top of the vessel, and is bent outwards, so that the cold water entering and falling to the bottom may distribute itself as it falls. The hot water pipe, on the other hand, ends at the bottom of the vessel, so that the hot water may rise and diffuse itself. In addition to this, the latter pipe is provided with a flexible tube, through which air is blown from the mouth, and

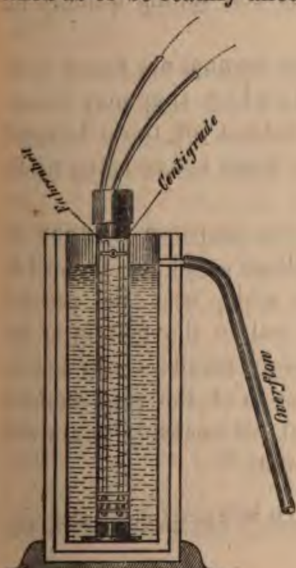
bubbling up through the water keeps it well mixed and of uniform temperature.*

When the deflection of the galvanometer needle is towards the left it indicates that the bath is too cold, and vice-versâ. The operator then adds hot or cold water, as the case may be, until the balance of electrical resistance is established, when the mercury thermometer gives a true reading of the temperature at the distant place.

By the use of a similar arrangement of apparatus and burying the resistance thermometers at various depths in the ground, the temperature may, without disturbing the coils, be registered with the utmost accuracy at different periods from year's end to year's end. In like manner, the temperature of the atmosphere at elevated points may be registered in a consecutive manner.

In constructing a thermometer adapted for measuring deep sea temperatures it was necessary to fulfil the following conditions:—(1) The resistance must increase or decrease with a higher or lower temperature, sufficiently to allow of an exact reading to one-tenth of a degree Fahrenheit. (2) The wire must be so protected mechanically that, under the pressure of a column of water of 3,000 fathoms, it would remain perfectly insulated. And (3) the wire must be so coiled as to be readily affected by slight changes of temperature in

Resistance coil protected against water.



* Since the above was written, I have adopted a modified arrangement of this apparatus shown in the annexed figure. It consists of a plain cylindrical vessel, into which a moveable tube is immersed, containing the coil and the mercury thermometer. A flange at the bottom of the tube serves to agitate the water in moving the tube up and down, and thus serves [to equalize the temperature of the liquid.

its vicinity. To effect this, a fine iron or copper wire, insulated with silk, is coiled in two or three layers upon the brass tube, aa, as shown in section in fig. 5. One end of this wire is soldered to the tube: the other to a copper wire insulated with gutta-percha

Fig. 5.

and carried through a hole to the interior. Over each end of the tube is drawn a piece of vulcanized india-rubber pipe, b and b′, in the space between which the wire is coiled. Over the whole is then drawn a larger india-rubber pipe, cc, which, after being padded outside with hemp yarn, is lashed tightly down by a stout binding wire. The gutta-percha covered wire forming the insulated end of the coil is placed between the india-rubber pipes, b and c, which are so compressed by the lashing as to close in upon it on all sides. The end of this wire is soldered to one of the leading wires; the other leading wire being soldered to the top of the brass tube. The whole is carried upon the end of the cable or sounding line, which contains the leading wires. The reason for leaving the interior tube open at both ends is to allow a free passage for the water through it, in order to ensure the coil taking quickly the surrounding temperature.

Thermometer coils constructed in this manner are found to be unaffected by any hydrostatic pressure to which they may be subjected. As a test of their insulation, I subject all those intended for deep sea soundings to pressure under water before being finally connected with the sounding lines.

An instrument of this description was prepared, in 1869, for the Dredging Committee, by which readings could be obtained to one-tenth of a degree of Fahrenheit's scale, with the greatest accuracy, in lowering the thermometer coil to the bottom of the harbour. Unfortunately, however, accurate results could not be obtained in deep water, because the motion of the ship rendered the needle of the galvanometer employed too unsteady to allow of dependence being placed upon its indications.*

* A similar apparatus has been taken out on board H.M.'S steam-ship "Challenger,"

The very high degree of heat to which pyrometers have to be raised, renders it necessary to construct them as nearly indestructible by fire as possible, and of a material which is not liable to any permanent change by sudden variations in and elevation of temperature. Platinum is a metal which is well suited for this purpose, in every way, as it does not, when annealed, alter its specific electrical conductivity by the application of heat; whilst the variation of its measured resistance, due to change of temperature, is sufficiently great to allow of exact readings. But special precautions had to be observed in providing a resistance wire of suitable quality, and in protecting the same from the hot gases of furnaces, which would exercise a chemical action upon it. **Resistance coil protected by platinum.**

The pyrometer coil which I prefer is made of fine platinum wire of 0·01 inch diameter, the resistance of which averages 3·6 units per yard of length. This wire is coiled upon a cylinder of hard baked pipe-clay in which a double threaded helical groove is formed, to prevent the convolutions from coming into contact with each other. The form of pipe-clay cylinder is shown in fig. 6.

Fig. 6.

At each end of the spiral portion, BB, it is provided with a ring-formed projecting rim c and c′, the purpose of which is to keep the cylinder in place when it is inserted in the outer metal case, and to prevent the possibility of contact between the case and the platinum wire. Through the lower ring c′, are two small holes, bb′, and through the upper portion two others aa′. The purpose of the upper holes, aa′, is for passing the ends of the platinum wires through, before connecting them with the leading wires. From

in her exploring expedition, in which a Thomson marine galvanometer was substituted for the more simple instrument used on the previous occasion, and which is better suited for taking readings notwithstanding the motion of the vessel. Considering that the zero position of the galvanometer has only to be ascertained, the difficulty of operating with this instrument would not be considered great by those who are accustomed to electric observations on board ship, although they are still considerable to the uninitiated in this class of observations, and renders the production of a more simple current detector a matter of considerable interest.

these two holes, downwards, platinum wires are coiled in parallel convolutions round the cylinder to the bottom, where they are passed separately through the holes bb′. Here, they are twisted, and, by preference, fused together by means of an oxy-hydrogen blow-pipe. At this end, also, the effective length and resistance of the platinum wire can be adjusted, which is accomplished by forming a return loop of the wire, and providing a connecting screw-link of platinum, L, by which any portion of the loop can be cut off from the electric circuit.

The pipe-clay cylinder is inserted in the lower portion AA, of the protecting case, shown in fig. 6. This part of the case is made

Fig. 7.

of iron or platinum, and is fitted into the long tube, CC, which is of wrought iron, and which serves as a handle. When the lower end of the casing is of iron, there is a platinum shield to protect the coil on the pipe-clay cylinder. The purpose of the platinum casing is to shield the resistance wire against hot gases, and against accident. At the points, AA, fig. 6, the thick platinum, wires are joined to copper connections, over which pieces of ordinary clay tobacco-pipe tube are drawn, and which terminate in binding screws fitted to a block of pipe-clay, closing the end of the tube. A third binding screw is provided, which is likewise connected with one of the two copper connecting wires, and which serves to eliminate disturbing resistances in the leading wires, as will be explained in the third part of this paper.

If temperatures not exceeding a bright red heat are to be measured, the platinum protecting tube may be dispensed with, and iron or copper substituted.*

* In experimenting with pyrometers with platinum casings, no appreciable deterioration of the platinum wire or change in its conductivity at 0° Centigrade has been observed, beyond what is due to the complete annealing of the wire in the first instance. With a view, however, of saving expense, the protecting tube of subsequent instruments was made of wrought iron; and an instrument of this construction was submitted for trial to a Committee apppointed by the British Association in 1872-3. To my surprise it was found that each time, after the coil had

The pipe-clay tube, upon which the platinum wire is wound, is, when cold, highly insulating; when heated, its conducting power increases, though not to such an extent as to occasion any perceptible error. Insulation of pipe-clay cylinder.

In order to investigate the extent of this increasing conductivity, I coiled a length of platinum wire round a pipe-clay pyrometer cylinder in the ordinary way between the leading wires, and then cut the wire at the bottom, so that the current passing between the leading wires would have to traverse the body of the pipe clay, and then measured its resistance at various temperatures, with the following results:—

Cold	1,000,000	units.
At intervals whilst red-hot .	12,000	"
	8,000	"
	7,000	"
	6,000	"
	3,700	"
	4,000	"
At white heat.	3,700	"
At intervals, in a gas furnace intensely heated	700	"
	650	"
	650	"
	550	"
	500	"

The resistance of the cylinder, when cold, returned to its original value, and after repeated experiment, produced the same results, whence it follows that the amount of error caused by con-

been exposed to intense heat, the platinum resistance at standard temperature was permanently increased; and, on examining the wire, it was found to present a rough surface, and had become brittle. Prof. A. W. Williamson, the Chairman of this Committee, suggested that this change might be owing to the reducing atmosphere produced by the highly heated iron casing, and which would cause the platinum to combine with a trace of reduced silicon, taken from the pipe-clay cylinder in contact with the same. An analysis by Prof. Williamson of the altered wire confirmed this view, and proved beyond doubt the necessity of an oxidizing or neutral atmosphere within the protecting chamber. This condition will be best obtained in making the protecting casing of platinum; but for ordinary purposes an iron casing well enamelled on the inner surface, or containing a lining of porcelain, will answer equally well.

duction of the pipe-clay cylinder, is practically inappreciable until a white heat has been reached: but that in measuring temperatures exceeding a white heat, it is the tendency of the instrument to indicate a slightly lower value than the true one.

In order to avoid inaccuracy from this source, it is desirable to expose the instrument to intense heat for three minutes only, on an average, at the end of which time the observation should be taken. This period of exposure will have sufficed to heat the protecting capsule, and the platinum resistance-wire, to within narrow limits of the full temperature of the furnace, whilst it will have been insufficient to penetrate and soften the pipe-clay cylinder. The error caused by an invariable and insufficient period of exposure is, moreover, proportional to the temperature, and can be determined by experiment at a temperature below white heat.

In adapting the resistance thermometer to the measurement of high temperatures, a wide range of resistances is obtained, and it is no longer necessary to determine these resistances with the same precision as in measuring slight variations of ordinary temperature. In this case I dispense with the use of galvanometers and substitute for the same an instrument which I propose to call a differential voltameter. The method of measuring electrical resistances by the aid of this instrument will be described in the Third Part of this paper—"On a simple method of measuring electrical resistances."

Although the principle involved in the increase of electrical resistances with increasing temperatures is an extremely simple one, the difficulties which had to be overcome in constructing practically useful instruments for measuring high and low temperatures, were considerable. Various combinations and appliances had to be tried for protecting the thermometer coils against hydrostatic pressure, or against the destructive heat of furnaces. The disturbing effect of leading wires had to be eliminated, and the reading of the instrument rendered independent of mechanical or magnetic influences, and brought within the compass of observers untrained for the delicate work of the electrician.

But the greatest drawback consisted in the imperfect state of electrical science respecting the ratio of increase of electrical resistance with increase of temperature, for temperatures ex-

ceeding the boiling point of water. Platinum is the only available metal for high temperatures, and little was known of the ratio of increase of this metal even at ordinary temperatures. I was, therefore, obliged to undertake the series of experiments, with the view of determining the increase of platinum resistance up to high temperatures, tending to the establishment of the general law with regard to electrical resistances—which has been dealt with in the First Part of this paper—"On the influence of temperature upon the electrical resistance of metallic conductors."

The resistance thermometer and pyrometer have already been applied to useful work. Professor Bolzani, of Kasan, uses them for registering cosmical temperatures at points above and below the surface of the earth. Mr. J. Lowthian Bell, the eminent metallurgist, employs the latter for determining the temperatures at which the various operations of the blast furnace are carried on; and I have had various occasions, in addition to the one already referred to, of obtaining useful information regarding the temperature of furnace gases, etc., by the aid of these instruments.

PART THIRD.

On a Simple Method of Measuring Electrical Resistances.

Resistance measurers and galvanometers.

Although the Wheatstone balance furnishes the electrician with the means of measuring the resistance of electrical circuits with great accuracy, provided only that reliable resistance scales and a delicate galvanometer are at hand, its application is, in many cases, rendered difficult on account of the delicacy of the apparatus and of extraneous disturbing causes.

In cases where a portable instrument is required which may have to be entrusted to inexperienced hands, the want of a more simple method of ascertaining electrical resistances makes itself particularly felt. Having had occasion to require such an instrument for measuring temperatures at inaccessible places, I projected, some years since, a "resistance measurer," which has been described by the Electrical Standard Committee of the British Association, in their report, at Dundee, of 1867, and which is based upon the power of balancing the potential values of two equal coils upon a magnetic needle, by changing their relative distance from it, according to the intensity of the two branch currents emanating from the same battery; this distance being made the measure of the unknown resistance inserted in one of the two branches.

Dr. Werner Siemens has produced a measuring instrument of greater scope and convenience, in which an index handle (moving a contact roller upon a wire in a circular groove) is carried round upon a divided scale until a magnetic needle in the centre of the apparatus assumes its zero position, when the unknown resistance is indicated upon the scale. The same instrument is suitable for measuring greater resistances by the sine method; it is also a tangent galvanometer, and has received the appropriate appellation of an "universal galvanometer."

These and other ready methods which have been projected for measuring electrical resistances are useful auxiliaries to the Wheatstone bridge, from which they differ chiefly in obviating the

necessity of elaborate resistance scales, without, however, removing the difficulty of dealing with a delicate galvanometer.

Professor Sir William Thomson has produced a marine galvanometer, which is nearly independent, in its action, of external magnetic influences and of the disturbing influence of the ship's motion. But these advantages are not realized without the sensitiveness of the instrument being, to a very great extent, sacrificed. By mounting the magnetic needle of the instrument upon a vertical spindle resting upon the end of a lever vibrating under the influence of a Neef's hammer, I succeeded in obtaining greater sensitiveness, but at the cost of a more complicated apparatus.

Theory of differential measurement.

At this stage of my inquiries, it occurred to me that both the resistance scales and the galvanometer might be dispensed with in measuring electrical resistances, by reverting to the principle of the voltameter in combination with that of differential measurement.

Faraday established the law that the decomposition of water in a voltameter in an unit of time is a measure of the intensity of the current employed; or, that

$$I = \frac{V}{t};$$

—I being the intensity, V the volume, and t the time.

According to Ohm's general law, the intensity, I, is directly governed by the electro-motive force, E, and, inversely, by the resistance, R, of the electric circuit, or, it is

$$I = \frac{E}{R}.$$

Combining the two laws, we have

$$V = \frac{E}{R} t,$$

which formula would enable us to determine any unknown resistance, R, by the amount of decomposition effected in a voltameter in a given time, and by means of a battery of known electromotive force.

Practically, however, such a result would be of no value, because the electromotive force of the battery is counteracted by the polarization, or electrical tension, set up between the electrodes of the voltameter, which depends upon the temperature and concentration of the acid employed, and upon the condition of the platinum surfaces composing the electrodes. The resistance to

be measured would, moreover, comprise that of the voltameter, which would have to be frequently ascertained by other methods, and the notation of time would involve considerable inconvenience and error. For these reasons the voltameter has been hitherto discarded as a measuring instrument, but the disturbing causes just enumerated may be eliminated by combining two similar voltameters in one instrument, which I propose calling a "differential voltameter," and which is represented in the accompanying drawing.

Differential voltameter.

It consists of two similar narrow glass tubes, A and B, of about 2·5 millimetres in diameter, fixed vertically to a wooden frame, F, with a scale behind them divided into millimetres or other divisions. The lower ends of these tubes are enlarged to about 6 millimetres in diameter, and each of them is fitted with a wooden stopper saturated with paraffin and pierced by two platinum wires, the tapered ends of which reach about 25 millimetres above the level of the stopper. These form voltametric electrodes.

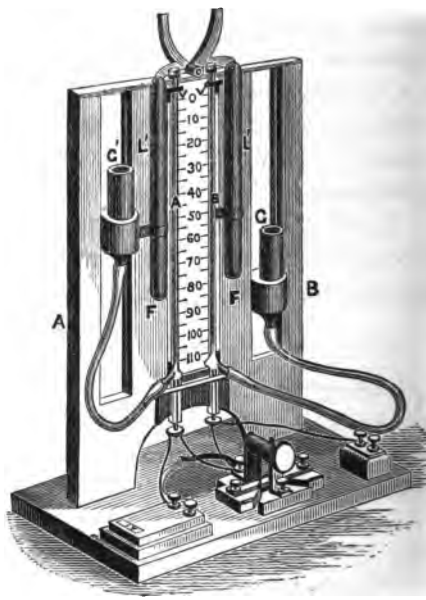


Fig. 8.

From the enlarged portion of each of the two voltameter tubes a branch tube emanates, connected, by means of an india-rubber tube, the one to the moveable glass reservoir G and the other to G′, fig. 8. These reservoirs are supported in sliding frames by means of friction springs, and may be raised and lowered at pleasure. The upper extremities of the voltameter tubes are cut smooth and left open, but weighted levers, L and L′, are provided, with india-rubber pads, which usually press down upon the open ends, closing them, but admitting of their being raised, with a view of allowing the interior of the tubes to be in open communication with the atmosphere. Having filled the adjustable reservoirs with dilute sulphuric acid,

on opening the ends of the voltameter tubes, the liquid in each tube will rise to a level with that of its respective reservoir, and the latter is moved to its highest position before allowing the ends of the tubes to be closed by the weighted and padded levers.

The ends of the platinum wire forming the electrodes may be platinized with advantage, in order to increase the active surface for the generation of the gases.

Pyrometer and voltameter connected.

Figure 9 represents the connections of the voltameter with the pyrometer, and also shows the necessity for the third leading-wire referred to at page 28 in the Second Part of this paper. One electrode of each voltameter is connected with a common binding screw, which latter may be united, at will, to either pole of the battery, whilst the remaining two electrodes are, at the same moment, connected with the other pole of the same battery; the one through the constant resistance coil, X, and the other through the unknown resistance, X′. This unknown resistance, X′, is represented to be a pyrometer-coil described in the Second Part of this paper.

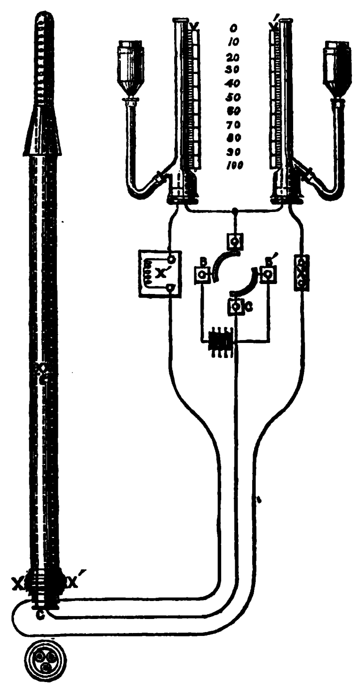


Fig. 9.

By turning the commutator seen at fig. 9 either in a right or left hand direction from its central or neutral position (in which position the contact springs on either side rest on ebonite), the current from the battery flows through the two circuits, causing decomposition in the voltameters; and the gases generated upon the electrodes accumulate in the upper portions of the graduated tubes. By turning the commutator half round every few seconds the current from the battery is reversed, which prevents polarization of the electrodes, as already stated. When through the position of the commutator the current flows from the copper, it passes

first through the connected electrodes to the voltameters, where it divides, one portion passing through the constant resistance, X, through the leading wire, X, to the pyrometer, returning by the leading wire, C, to the battery, the other passing through X′, through the leading wire, X′, through the platinum coil, returning by the leading wire, C, to the battery. When the current flows from the zinc it passes first through the leading wire, C, the current dividing at the pyrometer, one portion returning by the leading wire, X, through the constant resistance, X, through one voltameter tube to the battery, and the other through the platinum coil, X′, through the leading wire, X′, to the other voltameter tube, and thence to the battery. The value of the third leading wire, C, in eliminating the disturbing effect which long and short leading wires with varying temperature would certainly have upon the correct indications of the instrument is at once evident.

The relative volumes, v and v′, of the gases accumulated in an arbitrary space of time within each tube must be inversely proportional to the resistances, R and R′, of the branch circuits, because

$$v : v' = \frac{E}{R}\,t : \frac{E}{R'}\,t,$$

and, therefore,

$$v : v' = R' : R.$$

The resistances, R and R′, are composed, the one of the resistance, C, plus the resistance of the voltameter, A, and the other of the unknown resistance, X, plus the resistance of the voltameter, B. But the instrument has been so adjusted that the resistances of the two voltameters are alike, being made as small as possible, or equal to about 1 mercury unit, to which has to be added the resistances of the leading wires, which are also made equal to each other, and to about half a unit; these resistances may therefore both of them be expressed by γ.

We have, then—

$$v' : v = C + \gamma : X + \gamma,$$

or—

$$X = \frac{v}{v'}(C + \gamma) - \gamma \quad . \qquad (1)$$

which is a convenient formula for calculating the unknown resistance from the known quantities C and γ, and the observed proportion of v and v′.

The constant of the instrument (γ) is easily determined, from time to time, by substituting a known resistance for X, and observing the volumes, v and v′, after the current has been acting during an arbitrary space of time, when in the above formula, γ, has to be separated as the unknown quantity, giving it the form

$$\gamma = \pm \frac{v' X - v C}{v - v'} \quad . \quad . \quad . \quad . \quad (2)$$

The condition of equality between the internal resistances of both voltameters is ascertained by inserting equal known resistances in both branch circuits, when

$$v = v'$$

should be the result. Failing this, the balance is generally re-established by reversing the poles of the battery, the reason being that hydrogen electrodes are liable to accumulate metallic or other deposit upon their surfaces, which is effectually removed by oxygen.

Such reversals of current should be effected at frequent intervals during the observation. Should this not suffice to establish a balance of resistance, it will be necessary to push the electrodes of the voltameter of greater resistance a little further into the tube.

The constant resistance, C, of the instrument should, as nearly as possible, represent a geometrical mean of the range of resistances intended to be measured, because the greatest degree of accuracy is obviously obtained when the quantities, v and v′, are nearly alike. If the difference between V and V′ is very great, the constant γ introduces an error into the result, because $\frac{C + \gamma}{X + \gamma}$ is not equal to $\frac{C}{X}$ unless C equals X. In order to work this instrument between wide ranges of temperature, it becomes necessary to make C variable, and nearly equal to X. It is also obviously desirable to have γ very small as compared with X. Reliable observations can, however, be obtained between the limits of $v = 10\ v'$ and $10\ v = v'$, from which it follows that, with a fixed coil, $C = 10$ units, resistances may be measured (subject to correction for the disproportion introduced by the value of γ), between the limits of 1 and 100 units. In adding a reserve coil of 1,000 units, the scope of the instrument can be extended from 1 unit to 10,000 units. Greater accuracy for resistances between 50 and 500 units would, however be insured by providing a third resistance of 100 units.

Precautions necessary in using the instrument.

Certain precautions have to be taken to insure reliable results in using the instrument.

1. The dilute acid employed in both tubes should be of the same strength, a condition which is easily realized in preparing a standard solution of about 9 measures of distilled water for one measure of chemically pure sulphuric acid; to be kept in a bottle for replenishing the instrument when required. The moveable reservoirs being closed by a cork, with but a small hole for the admission of air, will rarely require replenishing.

2. When the instrument has been refilled or has not been used for some days, it is advisable to verify the equality of resistance of both voltameters and their connection by passing the battery current through them for some minutes with equal resistances inserted in each branch. If a difference between the volumes of gases should be observed, the binding screws and the pads of india-rubber closing the tubes should be examined and the experiment repeated. It is possible that an irregularity may be observed in the first trial, owing to a difference in the condition of polarity between the two sets of electrodes, which will disappear when both shall have been subjected to reversed currents proceeding from the same battery; the solutions will, moreover, be fully and equally saturated with gases, and absorption of the gases avoided.

3. The battery power used should be proportional to the resistances to be measured, viz.:—For resistances not exceeding 100 units, from 5 to 6 Daniell or Leclanché elements, which cause an active decomposition without sensibly heating the coils or effecting a partial insulation of the electrodes by excessive generation of gases; for resistances of from 100 to 1,000 units, the number of elements may be increased, with advantage to 15 or 20, and a still greater number of elements may be employed in measuring resistances exceeding 1,000 units.

It is not advisable under any circumstances to use less than five Daniell's elements, although active decomposition may be obtained with a less number, for the reason that the voltameter itself exercises an opposing electro-motive force by polarisation, which may vary under certain conditions from 1·1 to 1·3 Daniell's elements, and that these variations would exercise a sensible difference in the result if the electromotive force of the battery did not very decidedly predominate.

In using large battery power the heating of the coils has to be guarded against, which may, however, be easily done by arresting the current, in reversing it, from time to time, whilst allowing the gases in the tubes to accumulate until a sufficiently precise reading can be obtained. From two to four minutes duration of current will, under general circumstances, suffice to fill the tubes.

4. The india-rubber pads should from time to time be smeared with a waxy substance, to prevent escape of gas between them and the edge of the glass tube, and I find that paraffin answers well for this purpose.

5. The state of the barometer has no influence upon the reading of this instrument, because fluctuations of the atmospheric pressure affect both branches equally. A slight error through difference of pressure would, however, arise if the reading of the instrument were taken after the current had ceased to act, and the reservoirs were to remain in their elevated position opposite the zero point of the scale, exercising a hydrostatic pressure equal to the depression of the liquids in the tubes. In order to eliminate this source of error, the two moveable reservoirs must be lowered until a balance of levels is established on each side between the tube and its reservoir before the reading is taken. This being done, the weighted lever is raised from each tube for the discharge of the gases, and the moveable reservoirs are raised back to their zero position.

6. Although, by careful selection, two tubes of nearly equal diameter may be obtained, it would not be safe to depend upon such uniformity where accurate results are required. Each tube should, therefore, be calibrated, and provided with its own scale; and, in case of a tube having to be replaced, a suitable new scale should also be provided. The smaller the diameter and the greater the length of the tubes, the greater will be the accuracy of the observations; but a limit is here imposed, by the necessity of the gas-bubbles rising freely to the surface, which limit is reached in reducing the tubes to 2 millimetres of diameter.

A much smaller diameter would suffice, if the gases were merely to propel a water-column before them in a horizontal tube, but I found that under such circumstances the resistance of the liquid by adhesion to the sides, caused considerable error and inconvenience in the manipulation of the instrument.

Having measured numerous resistances by this instrument, and

compared the results with measurements obtained by a very perfect Wheatstone bridge arrangement, I find that it may be relied upon within one-half per cent. of error of observation, excepting at the extremes of the range, where a somewhat greater amount of error easily occurs unless special care be taken in reading the comparatively few divisions on the one side. A higher degree of accuracy is, in such a case, to be attained by filling the one tube several times (noting the volume each time), and allowing the other to continue accumulating, until at least 100 divisions of the scale shall have been passed.

A table has been prepared which gives the temperatures corresponding to the volumes of the gases of decomposition observed in the tubes, thus saving all calculation on the part of the metallurgist, or other observer.*

In using such a table, the temperature measured by the apparatus is found indicated at the intersection of the two columns of figures, expressing the volumes of gases observed in the two tubes V and V_1; these figures commence only with 40, because it is not considered advisable to take an observation until at least 40 unit volumes of gas have been developed in each tube. Care is to be taken that no leakage of gas takes places under the weighted cushions, which is easily observed in allowing the depressed columns to stand without lowering the reservoirs when the levels between gas and liquid should remain constant. Although the differential voltameter here proposed for measuring electrical resistances not exceeding the limits of metallic and earth circuits does not surpass, or even equal the Wheatstone bridge arrangement for accuracy, when the latter is carefully prepared, and in the hands of a skilful operator, it yet possesses advantages of its own which will, I trust, recommend it to the notice of electricians. One of

* The manner in which the equation of the curve of increase of resistance with temperature is applied to the construction of the table here referred to is the following: the coefficients of the platinum wire employed, that is, the quantities α, β, γ, have first to be calculated, from a series of experiments made for that purpose, with one unit of resistance at zero Centigrade. The constant of the voltameter γ has next to be obtained in the manner explained at p. 37, and the resistance X of equation (1), p. 36, has then to be equated with that of r, given at p. 9.

The following is the calculation employed for the construction of the tables. The constant C is equal to 17 units, the resistance γ to 2 units, the platinum coil in the pyrometer has a resistance of 10 units at zero Centigrade, and the coefficients of the platinum wire employed are $\alpha = \cdot 039369$, $\beta = \cdot 00216407$, $\gamma = -\cdot 24127$; then

its intrinsic advantages is, that it gives the resistance to be mea-

equating the values of the resistances as given by equations (1) of p. 9 and p. 36 respectively :—

$$10\ (\alpha t^{\frac{1}{2}} + \beta t + \gamma) = \frac{v}{v'}\ (17 + 2) - 2$$

$$\alpha t^{\frac{1}{2}} + \beta t + \gamma = \frac{1 \cdot 9v}{v'} - \cdot 2$$

$$\alpha t^{\frac{1}{2}} + \beta t = \frac{1 \cdot 9v}{v'} - (\cdot 2 + \gamma)$$

$$t + \frac{\alpha}{\beta} t^{\frac{1}{2}} + \left(\frac{\alpha}{2\beta}\right)^2 = \frac{1 \cdot 9}{\beta} \cdot \frac{v}{v'} - \left(\frac{\cdot 2 + \gamma}{\beta}\right) + \frac{\alpha^2}{4\beta^2}$$

$$t^{\frac{1}{2}} + \frac{\alpha}{2\beta} = \left\{ \frac{1 \cdot 9}{\beta} \cdot \frac{v}{v'} - \left(\frac{\cdot 2 + \gamma}{\beta}\right) + \frac{\alpha^2}{4\beta^2} \right\}^{\frac{1}{2}}$$

$$t^{\frac{1}{2}} = \left\{ \frac{1 \cdot 9}{\beta} \cdot \frac{v}{v'} - \left(\frac{\cdot 2 + \gamma}{\beta}\right) + \frac{\alpha^2}{4\beta^2} \right\}^{\frac{1}{2}} - \frac{\alpha}{2\beta}$$

$$t = \left[\left\{ \frac{1 \cdot 9}{\beta} \cdot \frac{v}{v'} - \left(\frac{\cdot 2 + \gamma}{\beta}\right) + \frac{\alpha^2}{4\beta^2} \right\}^{\frac{1}{2}} - \frac{\alpha}{2\beta} \right]^2$$

Substituting the values α, β, γ, and remembering that the formula is calculated for the absolute scale of temperature, the formula for the Centigrade scale will take the following form, which is that given at the foot of the table :—

T° Centigrade

$$= \left[\left\{ 877 \cdot 975 \times \frac{v}{v'} + 19 \cdot 070544 + 82 \cdot 738226 \right\}^{\frac{1}{2}} - 9 \cdot 0960553 \right]^2 - 274$$

$$= \left\{ (877 \cdot 975 \times \frac{v}{v'} + 101 \cdot 80877)^{\frac{1}{2}} - 9 \cdot 0960553 \right\}^2 - 274.$$

By means of this formula, the temperature of the resistance coil, which gives a ratio of volumes in the voltameter tubes greater than the maximum given in the Table can be calculated, and the constants required for the calculation have been given in the Table for that purpose. The following is an instance of its application, in which V = 127 and V′ 41 volumes:

log. 877·975	=	2·9434822
+ log. 127	=	2·1038037
		5·0472859
− log. 41	=	1·6127839
log. 2719·518	=	3·4345020
+ 101·80877		
log. 2821·32677 ÷ 2 =		3·4504534
log. 53·11616		1·7252267
− 9·0960553		
log. 44·0201047 × 2 =		1·6436510
		2
log. 1937·7	=	3·2873020
− 274		
1663·7 = 1664° Centigrade nearly.		

The resistance of 17 units in the voltameter is made of German silver wire, so that the variation of its resistance with that of atmospheric temperature shall be so small as not to affect the correctness of calculated results.

sured in "work done," which is independent of the momentary changes in the strength of a current, by charge or electrification, that influence the temporary reading of a magnetic needle.

It recommends itself for use on board ship, not being in the slightest degree influenced either by the motion of the vessel, or by the magnetic influence of its moving mass of iron.

Its simplicity of construction is such, that each part can easily be examined and verified.

It can be used satisfactorily by persons unaccustomed to the delicate handling requisite in dealing with galvanometers, and elaborate resistance scales; it is very portable; and lastly its cheapness of construction brings it within the reach of students and others, who might not be well able to afford an expensive apparatus.

The following tables of actual measurements of resistances, made by Mr. Lüdtge, Ph.D., shows the degree of accordance between the findings of this instrument, and those of a very complete Wheatstone bridge arrangement, which may be deemed satisfactory.

FIRST SERIES.

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.	Battery (Daniell's Elements).
0·2	0·2	0·0	0	5
0·5	0·5	0·0	..	..
0·8	0·8	0·0	..	..
1·2	1·21	0·01	..	..
2·0	2·0	0·0	..	..
4·0	4·0	0·0	5	..
6·0	5·95	—0·05	..	..
7·5	7·6	0·1	..	..
10·0	10·03	0·03	10	..
14·0	13·89	—0·11	..	..
20·0	20·0	0·0	..	6
27·5	27·47	—0·03	..	..
30·0	30·1	0·1	..	..
34·0	33·82	—0·18	10	6
42·0	41·9	—0·1	..	..
50·0	49·8	—0·2	..	..
54·0	54·0	0·0	100	..

FIRST SERIES—*continued.*

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.	Battery (Daniell's Elements).
60·0	60·	0·0	..	8
68·0	68·42	0·42	..	..
74·0	74·06	0·06	..	..
82·0	81·9	—0·1	..	..
90·0	90·0	0·0	..	..
95·0	95·2	0·2	..	..
100·0	99·97	—0·03	..	10

SECOND SERIES.

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.	Battery (Daniell's Elements).
0·5	0·5	0·0	5	8
0·8	0·5	—0·3	..	..
1·0	1·09	0·09	..	..
5·4	5·32	—0·08	..	..
10·0	9·82	—0·18	..	..
14·0	13·70	—0·30	..	..
50·0	48·83	—1·17	10	..
80·40	80·02	—0·38	..	..
98·00	98·20	0·2	50	..
105·00	105·15	0·15	..	..
130·40	130·40	0·0	..	9
156·00	155·80	—0·2	..	..
192·00	192·00	0·0	..	..
205·00	204·70	—0·3	..	10
240·00	240·14	0·14	..	..
284·00	283·95	—0·05	..	..
300·00	300·30	0·30	100	15
312·00	312·00	0·0	..	..
321·00	321·00	0·0	100	15
335·00	334·90	—0·1	..	..
350·00	350·02	0·02	..	..
385·00	385·00	0·0	.	..
400·00	400·30	0·3	..	..
425·00	425·00	0·0	..	..

SECOND SERIES—*continued.*

Resistance according to Wheatstone's Diagram.	Resistance according to Proposed Differential Voltameter.	Difference.	Constant Resistance. C.
432·00	432·00	0·0	..
465·00	464·70	—0·3	..
500·00	500·40	0·4	..
520·00	519·90	—0·1	..
557·00	556·70	—0·3	..
582·00	582·00	0·0	..
605·00	605·05	0·05	..
624·00	624·00	0·0	..
674·00	674·00	0·0	..
700·00	699·50	—0·5	..
723·00	723·00	0·0	..
750·00	749·80	—0·2	..
805·00	804·30	—0·7	500
846·00	847·00	1·0	..
906·00	906·00	0·0	..
928·00	929·20	1·2	..
1008·00	1006·50	—1·5	1000
1060·00	1062·00	2·0	..
1130·00	1130·00	0·0	..
1250·00	1254·00	4·0	1200
1300·00	1303·50	3·5	..
1340·00	1340·00	0·0	..
1410·00	1406·20	—3·8	1400
1470·00	1471·20	1·2	..
1500·00	1500·00	0·0	1500
1550·00	1553·00	3·0	..

SECOND SERIES—*continued.*

Resistance according to	Resistance			Constant

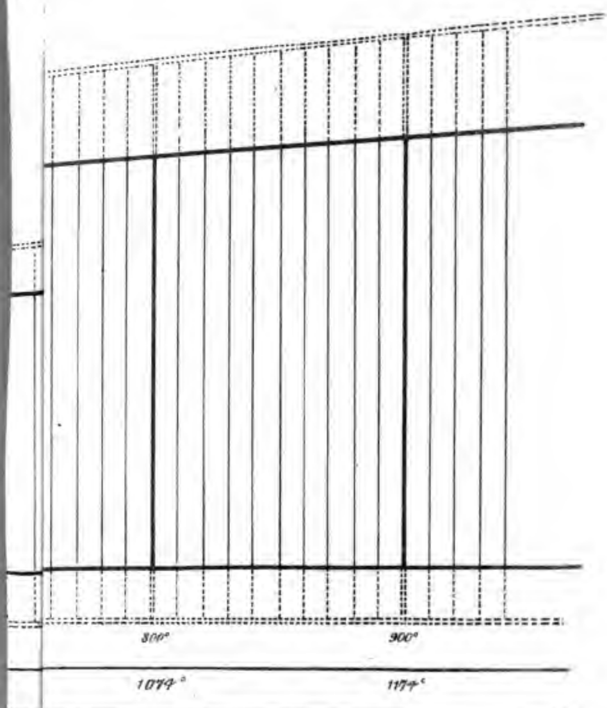
0216409

36 $T^{\frac{1}{2}}$ +

577 $T^{\frac{1}{2}}$

ver T =

Diagram No. 3.



N THE EFFECT OF VARIATION OF RESISTANCE SECONDARY WIRE CIRCUIT UPON THE AMOUNT ORK DONE IN A UNIT OF THE SAME BY AN ED CURRENT, THE CONDITIONS OF THE PRI- CIRCUIT REMAINING CONSTANT.

ell known that the work done in a unit of length of a r by discharge from a condenser is proportional to the f the quantity of electricity contained by the condenser. k done per unit of length should therefore not be affected otal resistance of the circuit. This can be proved experi- y by measuring the effect by the swing of a galvanometer and will be found true up to the limit of increase of resist- which the current is so much retarded as to admit of the aving sensibly moved before the whole quantity has passed. not find, however, that the effect of resistance on the induced s mentioned above has been brought to notice. The con- f the initiation of the current in the primary wire remain- stant, it seems reasonable to conclude that we have a electromotive force acting for a constant time t in our y wire.

ing as the expression for the value of the work done per unit sistance the formula $\frac{E^2}{R^2} \times t$ (where R is the total resistance), *ssuming E and t constant*, we get work $\propto \frac{1}{R^2}$. This should, if ssumption be correct, agree with the galvanometer measure of work done, *i.e.* work $\propto \sin^2\left(\frac{\theta}{2}\right)$ (θ being the swing).

n$^2\left(\frac{\theta}{2}\right)$ should therefore $\propto \frac{1}{R^2}$ or $\sin\frac{\theta}{2} \propto \frac{1}{R}$.

xperiment bears out this result, or in other words the sine of the swing is inversely proportional to the total resistance of ircuit.

This fact may be applied to the measurement of internal resistance of batteries, and appears to be superior to any of the methods which require the comparison of continued deflections or currents, more especially when "inconstant" batteries are under examination.

Such batteries will not maintain a constant electromotive force during the comparison of rather protracted and unequal currents, and the calculations are vitiated by its variation.

In my method I proceed as follows: viz. Divide the battery into two equal portions and couple the halves of the battery back to back, changing the cells until there is no great difference of potential between the terminals; place this nearly neutralised battery in a secondary circuit in connection with a reflecting galvanometer; adjust the shunt and directing magnet so as to keep the light on the scale whether the battery be in circuit or not; employ a few grove cells for the primary circuit, and compare the swings on closing the circuit (a) with the battery under examination in the secondary circuit, and (b) with a total known resistance substituted; then, calling these resistances $x + g$ and $a + g$ respectively, we have the proportion $\theta : \theta_1 = x + g : a + g$.

The induction is got between flat spirals in connection with the respective circuits, an iron weight being placed on top of them.

I would suggest also, with diffidence, that this method has advantages over those generally employed for the comparison of low metallic resistances, as we get over the unequal heating of the wires, occasioned by protracted currents.

This remark applies to the Wheatstone bridge when used without a Rheostat or other resistance admitting of continuous accurate increase or decrease.

R. Y. A.

DECAY OF TIMBER.

The following report was written and submitted to the Electric and International Telegraph Company on May 14th, 1860:—

"I have devoted considerable attention to the decay of our poles, and have endeavoured to arrive at some conclusion as to its cause. I think that I have hit the right nail on the head, and have not only traced the cause but have discovered a means to prevent this decay. I will describe my researches and explain my conclusions as succinctly as I can.

The decay of timber is divided into two kinds, the *dry* and the *wet rot.*

The dry rot is a slow, mouldering, destructive action that timber undergoes, by which its strength and fibrous structure are totally destroyed, and the wood itself becomes of the substance of snuff. It is usually attributed to the presence of sap in the wood, engendering, according to some, a species of fungus which in feeding on the wood destroys its texture, and, according to others, a chemical action similar to the oxidation of metals, &c. All kinds of timber are subject to it. Dry rot generally arises from painting or tarring young, unseasoned timber, which prevents the evaporation of the sap. The only effectual preventative is long seasoning, which process effectively removes by evaporation all the sap remaining in the wood. I have met a very few cases of dry rot in telegraph poles—certainly not more than 1 in 200 rotten poles is a case of dry rot. This arises from the fact that the pole in its exposed position is actually under a seasoning process. Dry rot, therefore, requires little attention, and I will proceed to the investigation of wet rot.

Wet rot is far more serious in its effects on telegraph poles. It may be considered practically to be the cause of the rottenness and decay of all our poles. Wet rot in its destructive effects upon the texture of wood is somewhat similar to the dry rot, but in place of reducing timber to a dry snuff it makes it a soft, wet, pithlike substance. It arises according to my views from the pores of the

wood becoming filled with water, which water on exposure to the atmosphere evaporates, and in evaporating disintegrates and tears to pieces the fibrous texture of the wood. Every pore of the wood becomes a little boiler, which bursts immediately the water is turned into steam. The process in a telegraph pole is as follows. The decay invariably commences at wind-and-water mark. It begins at the surfaces, and the wood gradually becomes disintegrated as we approach the centre. It extends 1, 2, 3, and sometimes 4 feet, but generally about 12 to 18 inches above and a few inches below the ground-line. Sometimes if the pole has been well tarred the decay commences and proceeds above the tar only. If the pole be taken out of the ground and examined it will be found saturated with moisture from the bottom to the spot where the decay occurs. If saw-cuts be made at different points from the sound timber to the bottom it will be found that as we approach the bottom the moisture increases. The butt-end being open, the pores of the wood which are exposed suck up the moisture by capillary attraction. The water gradually ascends until it comes to the wind-and-water mark, there, as soon as it becomes exposed to the variations of the temperature of the atmosphere, it evaporates and disintegrates the timber. This process goes on until the pole rots through. It thus appears that the evaporation of this moisture is the cause of the decay of our poles, and that to preserve our poles from wet rot we have evidently only to keep this moisture out and allow our poles to be dry. As this moisture enters the pole by capillary attraction through the pores of the wood, it is evident that if we stop up the ends of these pores we shall also stop this capillary action and thereby keep the pole dry. That this is the case is evidently proved by the fact that where our poles have been encased at the foot in iron screw sockets no decay has taken place. Iron sockets were used in some places on the Great Western and Bristol and Exeter lines, and those poles are now as sound as when they were first erected. This explanation of the cause of the decay of our poles will explain every peculiarity we experience in the maintenance of our poles—how the duration of timber in light sandy soils is different to what it is in thick clay—how some

timber is so much better than others. It also shows how futile was the system of asphalting at wind-and-water mark, and how useless any plan would be and has been which has been directed towards protecting the wind-and-water line alone, neglecting the bottom of the pole.

Now the mechanical plans which I propose for the prevention of decay to our poles are these—

1st. The use of iron screw sockets. This is known to be effective, but is very expensive.

2nd. The use of an earthenware socket, the empty space being filled up with asphalte. This has been proposed before, and is also expensive.

3rd. The use of a felt, earthenware, or iron shoe, filled with asphalte, pitch, or any impervious material at the foot of the pole, as per sketch. This is my process. I think that if we thus stop up the pores of the wood we shall stop the ingress of moisture *in toto*. Decay will cease, and our poles become durable.

The present system of charring and tarring should not be abandoned, but be very carefully attended to. I do not, however, perceive any great advantage in charring, and I think that if the bark be left on and well tarred it is quite as effective. The poles that I have recently prepared have been only barked to within 5 feet of the butt end. I have fitted on felt shoes and pitch to 3½ miles of poles on the Salisbury and Yeovil line. I think my plan very cheap, for it will not cost 6*d*. a pole.

Creosoting has been found very successful; but if my theory is correct it is evident that its success is due to the filling up of the pores with creosote in place of water. This would also be to some extent an explanation of the effect of boucherising, kyanising, or burnetising. But, as kyanising and burnetising have both proved only partially successful, so I fully anticipate that boucherising will turn out the same. I have little faith in its success, and object to it very much owing to its great expense.

There can be no doubt that seasoning timber is a very excellent plan, but I believe myself that, when protected by either of the processes I have named, it is of little consequence whether timber is

young or old. I believe that a tree just cut down from the woods would be as durable as one seasoned for years. In fact, as I said before, it is being seasoned in its position.

Again, I think that protected in this way Scotch fir and spruce would be quite as useful to us for poles as larch. Those two woods only decay quicker than larch because their pores are more open and can more readily imbibe moisture. The system is as applicable to splices and butts as to poles.

If the theory is correct my plan will prove most valuable to the Company. The maintenance and durability of our poles is a very serious matter, and any researches throwing light upon the cause or plan, reducing the expenditure and producing durability, must be considered interesting and valuable."

The experiment described was not successful, because the lateral absorption and vertical descent of the water were neglected.

Moreover, there is no doubt that creosote owes its success as much to its absorptive properties as to its mechanical function in filling up the pores of the wood.

W. H. PREECE.

ABSTRACTS AND EXTRACTS.

WESTERN UNION TELEGRAPH COMPANY.

The following is extracted from the Annual Report of the President (Hon. William Orton, M.Soc.T.E.) of the Western Union Telegraph Company to the Stockholders:—

The receipts for the year from all sources were 9,262,653.28 dols. and the expenses 6,755,733.83 dols. The difference 2,506,920.15 dols. is the net profit.

There have been added to the property of the Company during the year, by construction, purchase, and lease, 5,828 miles of poles and 21,264 miles of wire, being equal to about eight per cent. of line and twelve per cent. of wire; and 448 more offices were in operation at the close of the year than at the beginning. The Company operated at the close of the year 71,585 miles of line, 175,135 miles of wire, and 6,188 offices.

The International Ocean Telegraph Company.

The operations of this Company during the past year have been very satisfactory, and give promise of still better results in future. As I write, however, communication by cable between Punta Rassa and Key West is interrupted, but the necessary steps have already been taken to repair the cable, and it is expected this will be accomplished in a short time. The new cable between Key West and Havana, successfully laid the year before, has been paid for, and the entire floating debt of the Company discharged out of last year's earnings. There are now two good cables between those points. Unless it shall be found necessary, in order to insure permanent communication between the United States and the West Indies and South America, to lay an additional cable between Punta Rassa and Key West, it is probable that payment of dividends to the stockholders of the International Ocean Telegraph Company will be resumed within a year. This property is destined to increase largely in value in the near future.

General Review.

A comparison of the results of the Company's operations during the last fiscal year and the one preceding shows a reduction of 70,364.53 dols. in gross receipts, and of 251,042.54 dols. in the net profits.

This diminution of receipts and profits resulted from two causes; *first*, the reduction of rates, which took effect on July 1, 1873, pursuant to plans formed and instructions issued six months before; and *second*, to the financial panic of September, 1873, and the general stagnation in every department of business which immediately followed, and from which there has been but a partial recovery.

Commencing with July, 1873, the profits, as compared with the corresponding months of the preceding year, were less each month up to and including February, 1874, at which time the aggregate falling off for the eight months of the fiscal year was 589,564.09 dols.

For March the profits were in excess of March, 1873, and at the end of June the increase over the corresponding four months of last year amounted to 338,521.55 dols., leaving a deficiency of 251,042.54 dols. as stated above.

Although this report is for the year ending June 30th last, it seems proper to add, in this connection, that the profits for the first quarter of the current year, which ended September 30th, show an increase over the corresponding months of last year of more than 300,000 dols.

The fiscal year is from July to June, both inclusive. A comparison of the business of the calendar years 1873 and 1874 shows that the profits of the nine months of 1874, ended September 30th, are in excess of the twelve months of 1873; the excess during the seven months ended September 30th being 649,434.73 dols. over the corresponding seven months of 1873—an average increase of over 100,000 dols. a month.

The number of messages transmitted during the last year was 16,329,256, being an increase of 1,872,424 (about 13 per cent.) over the preceding year. Deducting from the gross receipts moneys received from other sources than for the transmission of messages, and dividing the remainder by the number of messages, it appears that the average receipt for each message was about 55 cents. As the charge per message is for a minimum of ten words, the average message must contain more than ten words; so that the average receipt per message is necessarily greater than the tariff fixed for a ten-word message. A uniform tariff of 50 cents.

per message of ten words between all stations on the Company's lines, without regard to distance, applied to the messages transmitted during the last year, would have yielded a revenue somewhat in excess of the actual receipts.

The tariff of rates now charged on the lines of the Western Union Company is but little above the average European rates. Considering the vast difference in the density of population, and the greater distances over which messages are sent in this country, and the cost of maintaining a greater length of line through sparsely settled sections, to reach the same number of people, and the higher cost of labour and of all material employed in telegraphic operations, the service in this country is relatively much cheaper than the average in Europe.

Duplex and Quadruplex Telegraphy.

The duplex apparatus of Mr. J. B. Stearns, by means of which two messages are transmitted in opposite directions upon one wire at the same time, has fully sustained the opinion of its utility and value which I expressed in my last Annual Report. It has been put in operation during the past year upon a number of additional circuits, and is now working successfully between all the principal cities. Its latest application was upon the lines to the Pacific coast, and it is now in use between Port Hastings, on the island of Cape Breton, where our lines connect with the cable wires, and San Francisco, a distance of nearly 5,000 miles.

But the past year has produced an invention more wonderful than the duplex. Mr. Thomas A. Edison and Mr. George B. Prescott, the electrician of the Company, have discovered processes and invented apparatus by means of which two messages can be sent in the same direction, and two other messages in the opposite direction simultaneously upon one and the same wire. This invention, which they have christened the quadruplex, has been in successful operation between our New York and Boston offices for the last two weeks, and is satisfactorily performing an amount of work upon one wire quite equal to the capacity of four wires worked with the ordinary Morse apparatus.

The inventors claim that the quadruplex may be used either as one wire, as two wires, three wires, or four wires, as the pressure of business may require; that when it is worked as two wires intermediate stations may be inserted, and may send and receive as with two separate wires in the ordinary way.

I have given much personal attention to the development of this invention, in the belief that if it could be utilized to the extent claimed by its inventors it would solve satisfactorily the most difficult problem which has ever been presented to the managers of Telegraph Companies, and that is: how to provide for the rapidly-increasing volume of business without an annual expenditure for the erection of additional lines and wires that would prevent the payment of reasonable dividends to stockholders. So much has been acoomplished already, and in so short a time, that it seems more likely that these predictions will be fully realized, than that the fulfilments will fall materially below the promise.

In my last Annual Report I made the following statement concerning the duplex apparatus:

"We are now operating more than 150,000 miles of wire, and during the past two years have been extending at the rate of nearly 20,000 miles of wire per annum. The duplex apparatus is capable of doubling the capacity of these wires at a comparatively small cost. The value of this increase of facilities can be approximately ascertained by estimating the saving in the investment for wire, and the annual saving in repairs and maintenance of additional wires. But the great value of the duplex does not consist in the saving in the investment in wires and the cost of repairs and maintenance, but in its ability to double the capacity of a wire when we have but one, and when no amount of money previously invested in wires, or even possible to be expended in repairs, can provide another."

These remarks will apply with even greater force to the quadruplex, if it shall prove capable of working through the same distances and under like conditions as the duplex. It is not easy to estimate the value of an invention which enables any and every wire between all the principal cities in the country, and between the Atlantic and Pacific Coasts, to be made equal to two, in a minute, by merely turning a button; but it is very evident that the ability to practically convert one wire either into two, three, or four, as the convenience or necessities of the business may require, is still more valuable.

The quadruplex, like the duplex, is partially substituted for, and worked in connection with, the Morse apparatus. No change in the ordinary operating force, nor any previous preparation of messages, is required, as with the automatic system, so that a continuance of the same simplicity and economy of manipulation and promptness of service

which have characterised the Western Union Company's system of telegraphy is assured. All the essential patents for the duplex are owned by this Company. Negotiations for the purchase of the patents of the quadruplex are pending, but the terms will not be settled until after the character and extent of its capacity for work have been more fully ascertained.

"Fast" Telegraphy.

This is the favourite designation given by its friends to what is better known as the automatic system. Why it should be called "fast"—in view of the fact that, before a message can be sent at all, more time must be spent in getting it ready for the transmission to begin than is required to send and deliver it in the ordinary way—I have never been able to comprehend.

In this review of telegraphic operations during the last year it is only necessary to say concerning "fast" telegraphy, that the progress of its development has been exceedingly slow. The latest attempt to utilize it in this country was made in 1869, on a line of one wire between New York and Washington, and now, at the end of five years, it stands about where it began.

Although the evidence which I have accumulated is not sufficient to convince me that automatic telegraphy possesses any value to the Western Union Company, in view of its control of the duplex, and of the probable utilization of the quadruplex, yet I have not failed to give careful attention to the subject, and, whenever it shall be demonstrated that any system of automatic telegraphy can be advantageously used on our lines, it will be promptly introduced. The claim that anything essential to the successful operation of automatic telegraphy—whether by the chemical paper plan of Bain or the later one of Wheatstone—is covered by controlling patents, is without foundation.

(*Phil. Mag.* Vol. XLVII. No. 307.)

ON THE DISINTEGRATION OF THE ELECTRODES IN THE GALVANIC ARC OF LIGHT.

By Hermann Herwig.

The electrodes experimented on were fixed air-tight in the ends of strong glass cylinders, from which the air was very perfectly exhausted by means of a Geissler air-pump. In the first experiments electrodes of iron, nickel, and copper were used. The current passing through the voltaic arc also passed through a voltameter, the object of the experiments being to determine what weight of metal was disintegrated from the electrodes for a certain volume of hydrogen evolved in the voltameter.

In the experiments with the above metals the results showed not the remotest approach to regularity in respect of the quantities disintegrated. These irregular results were proved to be owing to the disintegrated particles from one electrode becoming deposited on the other, and these particles becoming in turn disintegrated. The experiments were more successful when the electrodes were respectively a silver rod and a copper plate, the end of the rod being moved over and close to the surface of the plate, while the electric arc was kept up. A uniform disintegration of the silver rod was obtained. But the results showed—

(1) That the loss of silver is never even distantly equivalent to the amount of hydrogen developed;

(2) That the knobs suffer greater losses the more their form becomes modified by the action of the arc of light;

(3) Higher temperature of the knobs determines greater losses;

(4) That the positive electrode of silver suffers smaller losses when the opposite parts of the negative plate are dissimilar in constitution;

(5) That the knobs taken for the positive electrodes give greater losses.

A THEORY OF THE SOURCE OF TERRESTRIAL MAGNETISM.

By Professor Challis, M.A. F.R.S. F.R.A.S.

The author considers the effect of the orbital and rotatory motions of the earth in generating magnetic streams. The orbital or translatory

motion is discussed, and shown as likely to have but very little effect in generating these magnetic streams.

His theory attributes their generation from the rotatory motion to impulses given to the æther by the earth's atoms in motion, and this would be much corroborated by any independent evidence of the actuality of this action between atoms and the æther. The explanation of a certain phenomena of *aberration* of light gives this evidence. This *aberration* is due to the circumstance, that whereas the pointing of a telescope, as *instrumentally* determined, is in the direction of the straight line joining the optical centre of the object-glass and a certain fixed point in the field of view, the actual course of the ray from the first point to the other deviates from that line by reason of the earth's movement in the interval occupied by the passage of the ray. It was found experimentally that the filling of the tube of the telescope with transparent fluid did not affect the aberration. It would naturally be supposed that this would not be the case, in consequence of the rays being retarded in passing through the denser fluid. On applying the theory of impulses given to the æther by atoms, it was found that the effect of the dense fluid in the telescope should *not* be to affect the aberration of the light; the mutual action between atoms and the æther thus proving two diverse phenomena, as the aberration of light under the circumstances and the existence of terrestrial magnetism. Professor Challis points out the exceeding probability of his explanation being a correct one.

ON WHEATSTONE'S BRIDGE.

By R. S. Brough, Assistant Superintendent of Government Telegraphs.

In this paper Mr. Brough points out that bridges for testing telegraph lines are almost invariably wrongly arranged, and that when the galvanometer resistance is greater than the battery resistance (a condition which should always be fulfilled in testing telegraph lines) the galvanometer should be made to connect the junction of the two greater resistances with that of the lesser, or, in other words, that the galvanometer and battery should change places.

ON THE ELECTROMOTIVE AND THERMOELECTRIC FORCES OF SOME METALLIC ALLOYS IN CONTACT WITH COPPER.

By A. F. SUNDELL, Docent at the University of Helsingfors.

The forces were determined by the method of M. Edlund, which is based on the proposition, deduced from the mechanical theory of heat, that a galvanic current going through an electromotor calls forth in it an absorption or production of heat proportional to its electromotive force, according as the current is in direction the same as, or opposite to, that generated by the electromotor itself. Hence the relative quantity of the electromotive force is obtained by measuring the amount of heat absorbed or produced.

The amount of heat produced was measured by means of an air thermometer constructed by M. Edlund. It consisted of two copper cylinders connected by a horizontal glass tube. A thermoelectric combination of two metals was placed in either cylinder in such a position that the place of contact of the metals was at the centre of the cylinder, the openings in the top of the cylinder through which the metals passed being hermetically sealed. It is evident that any heating or cooling at the place of contact of the metals would cause the air in the cylinders to expand or contract. The metals in one cylinder were so connected with the metals in the other cylinder that on a current being passed through them one contact became heated and the other cooled. The trifling difference hereby produced between the temperature of the air in the cylinders occasioned a displacement of a column of liquid in a small tube. On reversing the current the contact which previously became heated now became cooled, causing a depression in the liquid column.

Each combination was tried with three different intensities of current. From the deflections the quantity α was calculated, which is proportional to the quantity of heat absorbed or produced with the unit of intensity (tan 45°) according to the equation

$$\alpha\, s = (\sqrt{\beta\, s^2 + 1})\, t,$$

in which β is a constant proportional to the galvanic resistance of the wire in the cylinder, s the current intensity, and t the deflection. The

constant β is obtained from the deflections t and t_1 at the intensities s and s_1, by the formula

$$\beta = \frac{(t_1 s + t s_1)(t_1 s - t s_1)}{s^2 s^2_1 (t + t_1)(t - {}_1 t)}.$$

The combination of each two of the three current intensities gave a value of β; in the calculation of α the arithmetic mean of the three values was inserted in the first equation. Several precautions to prevent loss of heat during the experiments, &c. were taken. The current was generated by 2 to 4 Bunsen's elements.

The *thermoelectric* forces were determined by placing the contacts of the metals in a test tube which was immersed in water, suitable precautions being taken to prevent error by currents sent up at the junction of the wires with the binding screws of the galvanometer used to measure the strengths of current set up by the cooled metals.

The results of the investigation were as follows,—the metals given being in contact with copper.

The quantities giving the thermoelectric forces were obtained by taking the deflections reduced to the temperature difference 10° C. and the conducting power 160 as relative measures of the thermoelectric forces:

	Electromotive force. (α)	Thermoelectric force.
12 parts bismuth 1 tin . . .	254·74	270·69
8 " " 1 " . . .	234·18	236·39
4 " " 1 " . . .	137·49	145·75
Iron	82·36	86·12
2 parts bismuth 1 tin . .	49·76	51·59
Copper	0	0
German silver	98·08	103·12
32 parts bismuth 1 antimony .	295·01	295·24
Bismuth	417·14	460·06
32 parts bismuth 3 antimony .	533·98	680·94

From these results M. A. F. Sundell shows that "a metallic alloy like the pure metals takes the same position in the electromotive force as in the thermoelectric series." He also states that the results are sufficiently near to justify his concluding that "the ratio between the thermoelectric and the electromotive force of the alloys investigated is constant and equal to the ratio for the iron-copper and copper-bismuth combinations."

ON A SIMPLE CONDENSER COLLECTOR FOR FRICTIONAL ELECTRICAL MACHINES.

By Samuel Roberts, Esq.

For a small machine with a cylinder 6 inches in diameter and 8 inches long the condenser was made of a glass tube $\frac{3}{10}$ inch in diameter and 13 inches long, hermetically sealed at one end. A copper wire of No. 25 gauge was inserted, and a piece of paper $2\frac{1}{2}$ inches broad is wrapped round the closed end four or five times. The open end of the tube with a little of the wire projecting is applied to the prime conductor, and the outer coating is in good electric connection with a metallic ball by means of a similar wire twisted once round the paper. Brilliant zigzag sparks $4\frac{1}{2}$ inches in length were obtained between the balls.

ON THE MOLECULAR CHANGES THAT ACCOMPANY THE MAGNETISATION OF IRON, NICKEL, AND COBALT.

By W. F. Barrett,
(Professor of Physics in the Royal College of Science, Dublin.)

The bars of nickel and cobalt experimented on were cylindrical, 9 inches long and 1 inch diameter.

On inclosing either of the bars in a helix of wire a sound was emitted as an interrupted current traversed the helix. The sound with the cobalt was found to be far more powerful than with either nickel or iron bars. It was found that the bar of iron and cobalt became elongated to the same extent when a current was passed through a surrounding helix. Little or no effect was produced on the nickel bar.

ON AN AIR BATTERY.

(*From "Proceedings" of the Royal Society.*)

By J. H. Gladstone, Ph.D. F.R.S., and Alfred Tribe, F.C.S.

This battery was composed of a silver tray, placed horizontally near the top of a jar filled with a solution of nitrate of copper, crystals of the

metal being placed in the tray, so as to rise in projections above the surface of the water. Below this plate a plate of copper was placed. These two plates formed the poles of the battery. With an oxygenized solution of the nitrate of copper a considerable electromotive force was obtained, but with a deoxygenized solution very little.

When the battery was placed under a bell glass containing air over mercury, it was found that the mercury gradually rose in the glass when the poles were connected by a wire, showing that the oxygen of the air became absorbed. A solution of nitrate of copper, containing six per cent. of the salt, was found to give the best effect. A combination of this kind, using copper and zinc electroides in an aërated solution of chloride of zinc, the zinc being placed so as to absorb oxygen from the air, gave an electromotive force equal to three-fourths of a Daniell cell.

(*Phil. Mag.* Vol. XLVII. No. 310.)

ON GALVANIC POLARIZATION IN LIQUIDS FREE FROM GAS.

By Dr. Helmholtz.

If a Daniell's zinc-copper element is closed by a water decomposition-cell with platinum electrodes, a current passes whose force gradually decreases as gas becomes developed in the electrodes. If the decomposition cell be disconnected from the battery, and the electrodes joined by a wire through a galvanometer, a gradually-decreasing current is observed, which finally dwindles down to nothing. The object of Dr. Helmholtz's experiments was to determine on what depends the apparently unlimited duration of the polarizing current. Since for the production of a changed equilibrium in a limited system of bodies (such as is the decomposition-cell) only a finite amount of work is ever necessary, the production of polarization must always give only a current of finite duration, or one whose intensity asymptotically approaches to zero, and the polarizing current could on the whole make only as much electricity flow in the one direction as the depolarizing in the opposite. So far as this is the case the decomposition-cell acts as a condenser, whose capacity is so great that it takes perceptible periods of time to charge and discharge, and whose dielectric is only an imperfect insulator.

It would be near to assume the same reason for the continuance of the charging current with a polarized decomposition cell as for the imperfect insulator of a condenser, viz. the existence of a slight metallic conducting-power in the electrolyzable liquid. Before such a conclusion can be drawn it must be ascertained whether other changes which might have similar results do not also occur in the liquid and electrodes. The chief of these changes Dr. Helmholtz finds in the power which metals have of absorbing gases into their substance. Thus, if two electrodes containing the *occluded* hydrogen be in circuit in a decomposition-cell with a cell of a battery, the oxygen developed at one plate by the action of the current will combine with the occluded hydrogen in that electrode, forming water; the hydrogen in the other electrode being increased; and inasmuch as the current in such a case has not to perform the work against the forces of the chemical affinity of hydrogen and oxygen, for the hydrogen occluded in one electrode in its readiness to combine with the oxygen there developed assists as it were this action. This electrolytic *convection*, as Dr. Helmholtz calls it, can be readily kept up by a feeble electromotive force which would be quite inadequate to decompose water. The same thing would occur if *oxygen* were occluded in the plates, the developed hydrogen readily uniting with it. Instead of being occluded in the electrodes, if the gas were occluded in the liquid the same effect would be produced. If then the electrodes and liquid in which they are immersed be well stored with oxygen, a continuous current could be kept up through them by a single galvanic cell. With hydrogen, which can be stored in very large quantities in the electrodes, a decomposition cell is found to act simply as a resistance, no polarisation being set up by the action of a current from a single cell on it. When the store of hydrogen in one of the electrodes runs short, hydrogen begins to be developed in bubbles at the plate to which it is carried, and therefore an apparent decomposition of water occurs.

(*Phil. Mag.* Vol. XLVII. No. 311.)

ON THE ELECTRIC RESISTANCE OF SELENIUM.

By the Earl of Rosse, D.C.L., F.R.S.

The experiments were made chiefly to determine whether the alteration of the electric resistance in certain experiments was due to light or

to radiant heat, as in a paper read by Lieutenant Sale before the Royal Society it is stated that the *actinic* rays produce no effect, but that it is a maximum in the red rays or beyond them. An experiment was made with a lighted candle placed $3\frac{1}{2}$ inches distant from the bar of silenium; the resistance fell 24·3 per cent. A vessel of hot water was then placed near the bar without producing any effect. Various films were placed between the source of light or heat and the bar, but the results obtained were the same. Experiments were then made to determine whether a suitable instrument for photometry could be constructed with a bar of selenium. The results showed that the decrease in resistance varied far more in proportion to the square root of the intensity of the incident light than to the intensity simply, although no exact law was obtained.

(*Phil. Mag.* Vol. XLVII. No. 314.)

ON WARREN'S METHOD OF FINDING FAULTS IN INSULATED WIRES.

By Thomas S. P. Bruce Warren, Electrician to Hooper's Telegraph Works, Limited.

The insulated wire is half wound on one drum and half on another; the two being perfectly insulated. The surface of the core between the two is perfectly dried. One end of the conductor is attached to one set of quadrants of an electrometer, the zinc pole of a battery being also attached to these quadrants. The other pole of the battery and the second set of quadrants of the electrometer are put to earth. The second end of the conductor is insulated. Earth is first put to one drum and then the other. The drum which causes the greatest fall of the electrometer contains the faulty portion of the wire. Wire is then coiled off that drum on to the other until the fault appears on the other. The position of the fault can thus be localised in a short time within a foot or two of the length.

H. R. Kempe.

"COMPTES RENDUS," *Tome LXXIX.*

No. 3 (*continued*).

FURTHER NOTES ON THE ELECTRIC CONDUCTIVITY OF LIGNEOUS BODIES.

By COUNT DU MONCEL.

By comparing the figures of the last experiments, as inscribed in the fifth column of the table that terminates the last communication, with those of former experiments, we see that the effects have been reversed. This appears to indicate that hard woods give up their moisture less readily than soft woods.

M. du Moncel, having tabulated further and very complete experiments with various woods, makes trial of the deflections to be obtained with plates of porcelain, glass, ebonite, resin, gutta-percha, and of paper. "The experiments commenced," the author continues, "at 9 p.m. with a humidity represented by 45° of the hair-hygrometer, and the plates remained exposed all night to the air, of which the humidity has passed successively through 45°, 48°, 39°, and 30°. At midnight I made a first series of experiments to determine the conducting power, with the aid of two large pieces of tin paper that I placed at 6 c.m. apart on each of the plates, which I put into communication with the galvanometer. I obtained no result in consequence of the humidity of the paper. Later, at 8 a.m., I obtained the following deflections :—

"Plate of glazed porcelain .	6°
" vitreous glass .	8°
" resin . . .	5°
" gutta-percha .	5·5°
" ebonite . .	5°
" thin paper . .	22°"

ON THE STRATIFICATION OF THE ELECTRIC LIGHT.

By M. NEYRENEUF.

It is possible to obtain stratifications of the electric light under the following circumstances, admitting the production with static electricity of inversions of charges as those given in the use of the Ruhmkorff coil.

Suppose two condensers connected in cascade with a H[illegible]z machine, [t]hemselves united by a Giessler tube instead of by a contin[illegible] [p]late. Placing the excitor of the machine in such a manner as [illegible] [s]mall sparks, these succeed each other with great rapidity. Two [illegible] [c]urrents, the one of charge the other of discharge, course along the [G]iessler tube, giving place to very marked stratifications. It is, if large [a]nd long tubes are used, to replace the ordinary small jars with some of [g]reater size. Those I have used contain 1873 c.m. □ surface.

No. 5.

ON THE THERMIC EFFECTS OF MAGNETISM.

By M. Cazin.

The memoir I have the honour to submit to the judgment of the Academy contains the detail of experiments which have led me to the laws of heat produced by magnetism in the core of an electro-magnet, and the description of new experiments having for their end the determination of *the magnetic equivalent of heat.*

Let m be the quantity of temporary magnetism of the electro-magnet, l the interpolar interval, ϕ the number of calorics created by the disappearance of this magnetism, A a coefficient constant for the same magnetising coil and the same arrangement of voltaic circuit, we have

$$A\,\phi = m^2\,l.$$

If the magnetism is employed only to produce heat in the core, the coefficient A will be constant, and will measure the number of units of *magnetic energy* equivalent to a caloric. This will be the *magnetic equivalent of heat.* To ascertain this it is necessary to measure ϕ and $m^2\,l$ in absolute units.

I have made two series of experiments to this end: the first to ascertain if A is a constant, the second to exactly evaluate ϕ. As to $m^2\,l$, I have previously obtained this value in absolute units.*

First Series.—In employing one or other of the two experimental methods I have indicated, I have observed the following facts :—

1. When the core of the electro-magnet is surrounded by two bobbins, of which one, the principal, receives a discontinuous voltaic current, and

* *Annales de Chimie et de Physique*, 4th series, vol. xxviii. 1873.

the other, the secondary coil, forms an induced circuit, the coefficient A varies with the method of closing this circuit. It is greater when the secondary circuit is completely closed. It has the same value while the circuit remains open and when it is closed only during the variable period of the closing of the primary circuit. It increases when the secondary circuit is closed only during the variable period of rupture of the primary circuit.

These facts show that the production of magnetic heat accompanies the disappearance of magnetism in the core, and that induction in the secondary circuit determines a division of the heat between the core and circuit.

2. The coefficient A diminishes with the duration of the rupture-spark. We vary this period in causing the spark to play across diverse media—air, water, alcohol, ether, etc.

3. When the point and the mercury of the interrupter are, respectively, connected with the armatures of a condenser, the coefficient A becomes very small. It diminishes as we increase the surface of the condenser to within a certain limit and as the number of turns in the coil is increased.

4. Whilst the condenser produces a spark between two metallic points, which communicate with its armatures, the value of A is much increased. In this case the condenser, having been charged by the rupture extra-current, discharges itself by the lateral circuit and by the bobbin. If this is of great length the first of these discharges is the more intense.

It results from this collection of facts that to measure the magnetic equivalent of heat it is necessary to employ a coil of short and coarse wire, to diminish as much as possible the induction of the coil upon itself, to employ a current of feeble intensity, to reduce the period of the rupture extra-current, and to produce the rupture-spark in a very resistant medium.

Regarding these observations I have arranged the calorimetric apparatus I have employed so as to furnish absolute values of the heat of the core.

Second Series.—The differential thermo-electric apparatus I have described* makes known the increments of pressure h from the air inclosed in a tubular magnetic core, with N interruption of the current.

It is easy to calculate the elevation of temperature θ that this air has experienced.

* *Comptes Rendus*, 23 March, 1874.

Let H_0, t_0 be the atmospheric temperature at the moment we adapt the manometer to the cylinders of the apparatus, t the temperature of the compensator cylinder at the moment of experiment, then we have

$$\theta = \frac{h}{H_0}(273 + t_0 - t).$$

The temperature of the air contained in the cylinder is that of the internal surface; it suffices to multiply θ by the weight of the iron P and by its specific heat C in order to obtain the number of calorics created by the magnetism.

An example of a determination of this kind follows:—

Number of turns in the coil	480
Cylindrical core (42 c.m. long, 2 m.m. depth, 25 m.m. radius) weighing	832·5 grammes.
Intensity of current (unit decomposing 9 milligrammes of water per second)	0·0232
Number of interruptions in 10 mins.	2778
Value of h observed in water	17·8 m.m.
Correction due to cooling	1·4
Value of θ	0°508
$\frac{\theta}{N} Pc = \phi$ (the caloric raises 1 kilogramme of water from 0^c to 1° c)	0^{c}0000174
Value of m^2l (the unit of magnetism is the quantity that, applied to a point, and acting upon an equal quantity, placed at a distance of 1 decimetre from the former, produces a force of 1 decigramme at Paris; the unit of length is one decimetre) .	1855

We deduce from this for a caloric—

$$A = \frac{m^2l}{\phi} = 106,000,000 \text{ units of magnetic energy.}$$

FOURTH NOTE ON THE CONDUCTIBILITY OE LIGNEOUS BODIES.

By Count du Moncel.

The present note has for its object the study of the variations of conductivity of wood according to its length and section. These measures are found to be extremely variable.

No. 6.

FIFTH NOTE ON THE CONDUCTIVITY OF LIGNEOUS SUBSTANCES.

By COUNT DU MONCEL.

Continuing this investigation; and as result the author finds the intensities of the current traversing the specimens of wood to be proportional to the square roots of the surfaces of the communication-plates (electrodes). The influence of the direction of transmission with relation to the direction of the wood fibres is also studied, and it is deduced, from experiments made with the current directed across and directed with the fibre, that the small difference in conductivity observed was due to other causes than the difference of direction of fibre.

STRATIFICATION OF THE ELECTRIC LIGHT.

By M. BIDAUD.

M. Neyreneuf's note on the stratification of the electric light, published in the *Comptes Rendus* of 20th July, induces me to make some remarks upon these phenomena. In April last, repeating the experiment of the electric light in rarefied air, I replaced the tube which serves to show the law of falling bodies in vacuo, as ordinarily employed, by a Giessler tube. One of the platinum wires communicated by a chain with earth, and the other was approached to the conductor of a Carré machine. I then obtained a luminosity as stratified as if I had connected the tube with the induced wire of a Ruhmkorff coil. Struck with this phenomenon, I observed it more closely, and increased the distances of the platinum wire from the conductor from 1 to 4 centimetres. Under these inductive conditions stratification was manifest in all its beauty. The charge of the conductor, and, consequently, the velocity of rotation, as well as the distance of the tube, influence the phenomena; but the moment of maximum intensity may soon be found.

No. 10.

SIXTH NOTE ON THE CONDUCTIBILITY OF LIGNEOUS BODIES.

By M. Th. du Moncel.

Having studied the modes of conductibility in wood, according to the greater or less hygrometric condition of the air and according to their nature, it remains to examine the manner in which we may render wood insulant. This question has a certain practical interest; for the sole non-fragile insulator we are yet acquainted with in the construction of electrical instruments is ebonite, and this substance is very dear, and subject after lapse of time to sulphurous efflorescence.

In a preceding communication I have shown that guaiacum and ivory, which were relatively rather good conductors, since they gave deflections of 50° and of 63°, became nearly insulant after passage through the stove and after immersion in certain liquids of a resinous or oleaginous nature. I explained this by saying that these liquids, being insulators and susceptible of modification by refrigeration, entered the pores of the two substances, prevented the moist air from ultimately penetrating, and, consequently, rendered the substances insulant. My supposition is realised by the fact that, after passage through the stove, the experimental pieces remained as insulant as ebonite and gutta-percha. Their nature seems to have suffered modification, for the ivory had become very brittle, and the guaiacum, which had changed its colour, had assumed a compact consistence analogous to that of ebonite. I have thus shown that certain ligneous substances, or substances of similar texture, may become insulant when they are so treated as to render them non-porous. It is curious that the sawdust of hard wood, agglomerated with blood and submitted to a considerable pressure in order to render it a solid and tenacious substance, as in the hard woods of M. Latry, constituted a very good insulator for voltaic currents. A piece of wood of this kind, sent me by M. Deschiens, did not furnish any deflection upon my galvanometer after thirty-eight hours' sojourn in my moist cabinet, although its surface had been covered by a heavy film, which was wiped off before the experiment. This property renders the wood very valuable in the construction of apparatus of precision, and I do not doubt that it may in many cases be substituted for ebonite. At the end of six days' sojourn in a very humid case it gave no deflection with the galvanometer.

After these results it would be supposed that the injection into ligneous

substances of insulating liquids, susceptible of solidification, would render the ligneous bodies insulant. In this belief, M. Niaudet-Breguet has sent me several samples of wood injected with paraffin; but as, with woods incompletely dried, the interiorly-imprisoned moisture after a lapse of time will repel the paraffin, giving relative conductivity, I asked M. Niaudet-Breguet to send me two series of paraffined woods, one prepared after passage through the stove, the other without this preliminary operation. When the samples of wood were remitted to me they presented no traces of conductivity, with the exception of the sample of mahogany, which gave a deflection of 12°. After twenty-two hours' stay in my moist cabinet I again experimented with them, previously wiping them; and I learned that the paraffined woods passed through the stove had become much less conductive than the others, as shown in the tables given later. In order to study the influence of the prolonged action of dry and of moist air upon these woods, I have exposed them to the sun during ten days in a dry chamber, and during another ten days in a very moist case. I have learned that, after losing their conductivity by drying, these woods, in spite of their paraffinage, suffered very sensibly the influence of humidity.

Desiring to assure myself if complete drying and an effectual paraffination produced better result, I passed through the stove for two hours the series of woods that had originally given the strongest deflections, and soaked them in molten paraffin for from half an hour to three hours. The results of trial after a subsequent sojourn of thirty-eight hours in the moist cabinet and six days in the case are given in the adjoining table (last two columns), and these results show amelioration of insulation. I especially observed that the paraffination in the last experiments had been effected in such a manner that the wood was coated with a layer of solidified paraffin, which was scraped off after cooling.

PARAFFINED WOOD AFTER PASSAGE THROUGH STOVE.

	After 12 hours in moist cabinet.	10 days in dry chamber.	10 days in moist case.
Poplar	16	2	55
Mahogany	10	0	65
Beech	8	2	58
Linden	8	2	72
Elm	8	0	40
Ash	6	0	56
Pear	6	0	43
White Deal	6	0	39
Walnut	2	0	52
Oak	0	0	26

WOOD PARAFFINED WITHOUT PASSAGE THROUGH STOVE.

	After 12 hours in moist cabinet.	10 days in dry chamber.	2 hours in stove and 36 hours in moist case.	Repassed through stove, re-paraffined, and 36 hours in damp case.	Again paraffined, and 6 days in wet case.
Poplar	18	4	21	10·0	1·5
Mahogany	35	24	24	8·0	3·0
Beech	18	3	25	1·3	0·5
Linden	22	0	35	6·5	3·0
Elm	10	0	17	1·3	0·5
Ash	22	6	27	13·0	5·5
Pear	16	3	21	6·0	1·0
White Deal	9	0	0	2·0	1·5
Walnut	9	0	8·5	1·3	0·5
Oak	5	3	5·5	1·0	0·5

I wished to study the influence produced by the penetration of other insulating liquids and by varnishing. I commenced by immersing my new-paraffined specimen of poplar in a solution of gum lac in alcohol; this specimen when dry was put into the case for four days and then gave 90° deflection; it gave previously 2°. My specimen of mahogany, which gave a deflection of 1°, coated with a layer of varnish, was placed in the cave for three days, and gave 0·5° while a similar specimen, unvarnished, gave 11°. Varnish augments the insulation of wood less when it is old; for a small plate of mahogany varnished and kept some length of time in a dry apartment gave a deflection of 9°.

I sought to know if powerful compression would render wood insulant by closing its pores, and I concluded that compression has the effect of augmenting the conductivity of the wood by giving greater continuity to the humid conductor and by expelling humidity to the exterior; but, once this action is produced, as the humidity of the atmosphere can no longer penetrate so easily the pores of the wood, the conductivity gradually diminished, in spite of augmentation of humidity in the surrounding medium.

PAGET HIGGS, LL.D.

ON THE IMPORTANCE OF A RATIONAL GROUPING OF THE ELEMENTS OF A BATTERY IN ELECTRICAL APPLICATIONS.

By M. TH. DU MONCEL.

We know that Ohm, in order to establish the best conditions of size of a battery with relation to a given circuit r, supposed the surface of a

battery of the resistance n R divided into n equal parts in such manner as to constitute a battery of n small elements in tension, giving for the expression of the intensity of the current

$$\mathrm{I} = \frac{n\,\mathrm{E}}{n^2\,\mathrm{R} + r},$$

a formula susceptible of a maximum, and of which maximum the conditions correspond to $n^2\,\mathrm{R} = r$, that is to say, to the equality of the two resistances forming the circuit.

This formula does not exactly agree with the method of grouping the elements of a battery, and I have sought a more practical expression, and have arrived thereat by the following reasoning: Let n be the total number of elements of a battery that we can arrange as we may wish. Let b be the number of elements united in quantity to constitute a group, and let a be the number of these groups. Since n is the total number of elements, $a \times b = n$. The quantities a and b may take any desired value from 1 to n, according to the conditions of experiment, but these should include whole numbers, and such that $a \times b = n$. If this be not the case, we should adopt the combination that will furnish for a and for b values approximating most closely the experimental conditions, and at the same time are most approximate n when intermultiplied. In the case where $a = 1$, and where $b = n$, all the elements are arranged in quantity, and in the case where $a = n$, and $b = 1$, all the elements are arranged in tension.

Let us suppose, however, that we have a group composed of b elements in quantity; the value of the intensity of the current furnished by this group, according to Ohm's law, will be—

$$\mathrm{I} = \frac{\mathrm{E}}{\frac{\mathrm{R}}{b} + r} = \frac{b\,\mathrm{E}}{\mathrm{R} + b\,r}.$$

If we reunite a number a of this group in tension so as to employ the total (n) elements, Ohm's law will give for the intensity I of the current—

$$\mathrm{I} = \frac{a\,\mathrm{E}}{\frac{a}{b}\,\mathrm{R} + r} = \frac{a\,b\,\mathrm{E}}{a\,\mathrm{R} + b\,r} = \frac{n\,\mathrm{E}}{a\,\mathrm{R} + b\,r}.$$

Now this formula, which represents generally the intensity of a battery whose elements are arranged in series, that is to say, by groups, is susceptible of a maximum, for, b being equal to $\frac{n}{a}$, the denominator of the

last formula becomes $a\,R + \frac{n\,r}{a}$, and is susceptible of a minimum. Indeed, the result of this expression, which is $R - \frac{n\,r}{a^2}$, being put equal to 0, gives $a^2\,R = n\,r$, or $a\,R = \frac{n}{a}\,r$, or $a\,R = b\,r$; or again, $\frac{a}{b}\,R = r$; this shows that the resistance of the circuit r should be equal to the internal resistance of the battery $\frac{a}{b}\,R$. Consequently, we can always with a battery composed of several elements obtain for a given exterior circuit r a voltaic combination susceptible of furnishing a maximum electric *intensity*, without regard either to the work produced or to Joules's formula.

These conditions of maximum being once established, it is easy to immediately ascertain the values of a and of b, to which they correspond. In effect, from the equation $a\,R = b\,r$, or $a\,R = \frac{n}{a}r$, we deduce—

$$a = \sqrt{\frac{n\,r}{R}}, \text{ and } b = \sqrt{\frac{n\,R}{r}}.$$

Values that may be obtained under another form, when the intensity I is given, by taking the formula $I = \frac{n\,E}{a\,R + b\,r}$ placed under conditions of maximum, which will become—

$$I = \frac{n\,E}{2\,a\,R}, \text{ or } I = \frac{n\,E}{2\,b\,r}$$

equations, whence we deduce—

$$a = \frac{2\,I\,r}{E}, \text{ and } b = \frac{2\,I\,R}{E}.$$

For what remains, we can easily learn the limits between which may with advantage be employed such or such method of grouping; for these limits, in order to the coupling of b elements in quantity, are—

$$\frac{n\,R}{(b-1)\,b}, \text{ and } \frac{n\,R}{(b+1)\,b}.$$

We will now turn our attention to the consequences of these different laws in electric applications.

Let us suppose that two telegraphic electro-magnets, each of 2,200 metres resistance, and wound with No. 16 wire, are applied directly to a Daniell battery of 8 elements arranged in tension. The force obtained by each of them will be about 70 grammes at an attractive distance of

1 m.m., consequently the total force they will develope will be 140 grammes. Now, if instead of two electro-magnets we employ only one, this force will be 200 grammes. We shall then lose 60 grammes by having two electro-magnets. At first sight this would appear an anomaly, or due to imperfect contact in the electrical connections, were it not that calculation leads to the same results.

Indeed, in applying Ohm's formulæ to the two cases quoted, we find—

1. That with one electro-magnet the intensity of the current has for its value (representing the electro-motive force of the Daniell battery by 5973) the number 4·95.

2. That in the case of the two electro-magnets constituting two equal derivations this intensity is represented by

$$\frac{8 \times 5973}{2(8 \times 931) + 2200} = 2{\cdot}79.$$

The forces of the electro-magnets in the two arrangements will then be to each other as the squares of the quantities 4·95 and 2·79. However, if we admit that the force of the electro-magnet put into simple circuit will be 200 grammes, the force of each of the two others will be given by the formula—

$$\frac{\overline{2{\cdot}79}^2 \times 200}{\overline{4{\cdot}95}^2} = 64 \text{ grammes};$$

consequently, the simultaneous force of the two electro-magnets will be 128 grammes instead of 200, as one of the same resistance would furnish.

This effect is consequent upon the arrangement of the battery which is already not in relation with the resistance of the exterior circuit, since it is nearly four times greater than the latter, and is still less with relation to the circuit as constituted with two derivations. The formula $\frac{n\,\mathrm{E}}{2\,n\,\mathrm{R} + r}$ (which represents in this case the intensity of the current on each derivation, and is in the case of a battery arranged in series for conditions of maximum $2\,\frac{a}{b}\,\mathrm{R} = r$ or $\frac{a}{b}\,\mathrm{R} = \frac{r}{2}$) shows that in this case the resistance of the battery should be half that of the circuit r or of the electro-magnet. Consequently the number a of elements in tension which should compose the battery will be furnished by the formula

$$a = \sqrt{\frac{n\,r}{2\,\mathrm{R}}}.$$

which in the conditions of the experiment we have cited would become

$$a = \sqrt{\frac{8 \times 2200}{2 \times 931}} = 3{\cdot}074,$$

and the number of elements in quantity should be $\frac{8}{3{\cdot}074} = 2{\cdot}6$. But, as the two numbers are not integral, it is necessary to adopt a combination which will give to the quantities a, b, and n values approximative to the given and calculated values, and we see that a battery of three elements in tension, of which each of the elements is composed of three elements in quantity, best unites the conditions. Now, with such a battery the force of the single electro-magnet will be 267 grammes, and that of each of the two electro-magnets introduced in the two derivations will attain 158 grammes. The total force of these two electro-magnets will be 316 grammes, and this time we gain by using two electro-magnets.

We thus can perceive how very important it is in electrical applications to be precise as to the experimental conditions in order that calculation may guide us to the best arrangement to give to the several members employed.—*Journal Télégraphique.*

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The Twenty-eighth Ordinary General Meeting was held on Wednesday, the 11th November, 1874, Mr. LATIMER CLARK, Vice-President, in the Chair.

The CHAIRMAN: I have to announce that some progress has been made towards the acquirement of the library of the late Sir Francis Ronalds. I think you have been already informed that Sir Francis Ronalds, with whom we were in communication on the subject during his life-time, has bequeathed a very large and valuable collection of electrical works to his relative, Mr. Samuel Carter, with full powers to dispose of them as he thought fit. That gentleman has already submitted to us the draft of an agreement by which the library will be transferred to the Society of Telegraph Engineers in trust, subject to certain conditions. Among those is one condition, that we should print a catalogue of the library, and publish it. Now that is rather a large undertaking, and will cost between £200 and £300, but I am happy to say one not at all beyond our means. The library, which is one of great value and interest, will be transferred to the Society in trust, subject to a condition that after the death of certain specified persons, who will probably be members of the House of Peers, it will become the absolute pro-

perty of the Society of Telegraph Engineers, if then existing. If, however, the Society should dissolve in the meantime, those trustees would have power to transfer it to the Royal Society, or to deal with it in some other way. I have great pleasure in making this announcement, for the library is one of the most complete collections of electrical works that the world possesses.

The following Paper was then read—

ON FAULTS IN SUBMARINE TELEGRAPH CABLES.

By J. J. Fahie.

I propose to-night to lay before you much perhaps with which you are already familiar, but still something that I believe to be new, and, as I hope you will admit, not unimportant, on the subject of Faults in Submarine Telegraph Cables.

About eighteen months ago I began a series of experiments with the object of discovering, if possible, a method by which the position of a fault in a submarine cable could be more accurately determined than at present. It is to a narration of some of these experiments, and of the conclusions I gather from them, that I would beg your attention.

At the period to which I refer, while considering where and how to begin my inquiries, I recollected having read in a justly-popular work on telegraphy that the voltaic current set up in a broken cable is always steady and positive. I knew that this did not apply to a cable not broken, but simply where the conductor is bared or exposed; still I resolved to begin by proving the fact. I had fortunately at my service a very faulty section of the first Persian Gulf cable, and upon this I at once proceeded to operate.

On putting the cable to earth through a galvanometer, I found that the current coming from it deflected the needle in the direction which showed that it was a negative current, and on observing

attentively for some time I could also see it was not steady. Lest these results may have been due to some accidental circumstances, I made a long series of similar observations, a few of which will be found in the Appendix to this paper. These observations were made at all hours of the day and night on four different sections (two of which were faulty), and were extended over a period of several months. They all tended to but one conclusion, viz., that the currents in bared cables cannot be distinguished in the matter of strength, except perhaps at times, from the ordinary earth-currents to be found in all telegraph lines, good or bad, whether over or under ground. As a proof of their identity I invariably found, that, when the current from the faulty cable was positive, the earth-current from the cable lying in the same direction was also positive, while those from the cable and land-line lying in a different direction were negative, and *vice versâ*. I also found it made no difference whatever whether the distant end of the line was insulated or not. Thus repeatedly, while watching the galvanometer, I caused the distant end to be alternately insulated and put to earth without producing the slightest effect on the needle. This, however, I think is true of very faulty cables only, for I have noticed in some cases, that, when the fault itself has a considerable resistance, the directions of the current from the cable, when the distant end is insulated and to earth, do not always coincide.

Having convinced myself that the cable-current could not be turned to any useful purpose on account of its variability, I set about some other experiments. Amongst the rest I connected the faulty cable above mentioned to a Wheatstone's bridge, and tested in the usual way its insulation resistance, using sixty cells negative, and noting at short intervals the direction and strength of the cable-current. The following is a specimen of the results obtained :—

Duration of testing battery contact.	Insulation resistance.	Remarks.
After 1st minute	587 units	Cable-current negative.
„ 15th „	570 „	ditto ditto but less strong.
„ 24th „	550 „	ditto ditto ditto.
„ 30th „	550 „	ditto rose slightly between 24th and 30th minute.
„ 41st „	520 „	Apparently no cable-current.
„ 47th „	500 „	Cable-current positive.
„ 50th „	496 „	ditto ditto.
„ 53rd „	494 „	ditto ditto slightly stronger.
„ 60th „	500 „	ditto decreasing.
„ 68th „	518 „	ditto ditto.
„ 75th „	512 „	ditto rose again.
„ 85th „	520 „	Apparently no cable-current.

The fault in this cable as afterwards discovered was actually 493 units of resistance from me, but, as we could get at this time some sort of signals from the distant station, the fault itself must have had some resistance. 520 units must consequently have been, if not the exact, at all events a very close, approximation to the real resistance of the line to and including the fault. Now, as 520 units was the resistance when there was no cable-current, it was obvious that if we could in every case eliminate this current the nearest approximation would be obtained. But the difficulty was how to do this at once and whenever required, for to wait until the current itself in its ebb and flow arrived at its lowest point or zero would indeed entail weary and, it may be, prolonged watching.

Mr. Culley recommends for this purpose the application of a strong negative current for ten or twelve hours, interrupting it occasionally, and giving a short positive current, and watching till the cable-current ceases. I tried this method, but without success. I cannot say how it would answer in the case of faults caused by actual breaks of continuity (indeed Mr. Culley describes it in con-

nection with such cases), but, so far as my limited experience goes, it is of little value for fixing with accuracy the position of a fault where the copper wire is merely exposed and not broken. The method fails mainly, as I think, because it is employed without any reference to the condition of the cable-current at the time of testing. Thus, bearing in mind that the current from a faulty (bared) cable is identical with the earth-current, it may, and in all likelihood will, so happen that it makes several oscillations from positive to negative, and *vice versâ*, during the ten or twelve hours the strong negative current is on line; and, therefore, unless the galvanometer be closely watched, the needle may pass several times over the zero point without being detected. Again, if at the time of testing, the cable-current be positive, the application of a negative current of a certain strength will be useful up to a certain point, but, if continued beyond this, it will do much harm. If, on the other hand, the cable-current be negative, a negative battery-current will only make it more so. For example, the current from my faulty cable was 5° negative; I put on sixty cells negative for five minutes, and on removing it from the cable-current was 55° negative, at which it remained steady for a long time, and then fell gradually to its first position.

By attentively observing the behaviour of the cable-current when left to itself, and when alternately opposed and assisted by the battery-currents, I was led to the following method of elimination. As I shall have frequently to refer to this hereafter as a part of my system of testing, I may be allowed, for the sake of distinction, to refer to it as "my method."

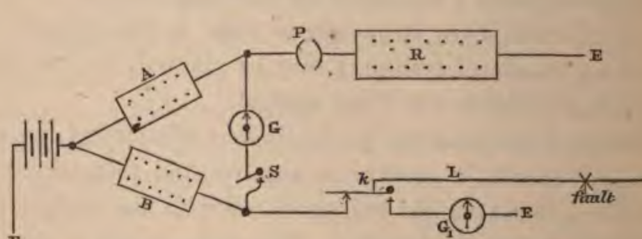
My method then consists, briefly, in first eliminating the cable-current, whether it be positive or negative, and then taking while the cable is free a simple resistance-test with the bridge, in the manner to be presently described. The cable-current is eliminated by sending into the line a current of the opposite sign to that coming from it, and arranging the strength and duration of this current to suit the strength of the one from the cable. Thus, if the latter be strong and negative, I put (say) sixty cells positive to line for a couple of minutes, and then note the condition of the cable-

current; if it be still negative, but weaker, I put the battery on again for a short time, and continue to do so until the galvanometer needle indicates a weak positive current from the fault. If the latter be now left to itself and the cable put to earth through a galvanometer, the needle will steadily, and as a rule leisurely, fall to zero and pass over to the other side, indicating a negative current again from the fault. While the needle is on zero the line is free and in a fit state for the subsequent test.

If the cable-current be positive, I put sixty cells negative on until I have depolarised the fault; the effect in this case is more brief than in the other, the needle falling quickly to zero and crossing to its original position.

Having once eliminated the current from the fault (and the operation never exceeded ten minutes in the most obstinate cases I have met with) the cable can always be kept free by momentary applications of the necessary battery pole. Thus, if the needle begin to move off zero in the direction indicating a negative current from the fault, a positive current applied for a moment will bring it back, and *vice versâ*. In practice I prefer to repolarise the fault slightly in the opposite direction, as I thereby gain a little time to arrange the bridge for a test.

Having shown how to prepare the cable, I will now describe the test. The bridge is arranged somewhat in the following manner:—



P is a circuit-breaker; when the plug is removed the connecti between the branch coils A and the resistance-box R is severed; *k* an ordinary key for putting the line to earth through the galvano

meter G′ or to the bridge as may be required. The rest needs no explanation.

I first ascertain by an ordinary test the approximate resistance of the faulty line L, and leave it unplugged in R. I next allow the line to rest for a few minutes in order that it may recover itself from the effects of the current employed in this preliminary test, and then depress k, and observe the cable-current on the galvanometer G′. Let it be positive, I open the switch S, remove the plug P, and send a negative current from my testing battery of (say) sixty cells into the cable *viâ* the branch-coils B, which should be plugged in to avoid heating. When I have repolarized the cable-current—a fact which I ascertain by putting L to earth at intervals through G′—I arrange the bridge, close the switch S, and, keeping L to G′, watch till the needle comes to zero; at that moment I let K fly back and send a negative current through the bridge system, observing the instantaneous effect on the galvanometer G. If R be too great the needle will be deflected in a direction (say to the right) indicative of this, but immediately after it will rush across zero and up the other side of the galvanometer (to the left), showing that the cable-current has again set in. If R be too small the needle will pass to the left, at first slowly, but immediately after with a bound, and knock against the brass pillar at 90°. R is now adjusted, resistance is inserted or removed as required, and the eliminating process begun again. As R more nearly resembles L, the first and instantaneous deflections after battery-contact become smaller; and, when R and L are equal, the needle trembles over the zero-point for a moment, and then rushes over to the left under the influence of the cable-current.

Should the current given off by the fault be negative, having arranged the bridge as before, I repolarise the fault with a positive battery current, and, waiting till G′ shows the cable free, proceed to test as before, but using a positive current instead of a negative. Should R be too great the needle of G will be deflected in this case, first to the left and then to the right. Should it be too small the needle will move to the right, at first slowly, but immediately after with a rush. The galvanometer G must always be ready,

and not short-circuited, else the first and instantaneous deflections after battery-contact will not be perceived.

From the 5th January, 1873, the day on which I began to apply this method of testing, to the middle of February following, I invariably obtained 507 units as the insulation resistance of the faulty cable; while the resistance obtained by the ordinary style of testing varied between 450 and 700 units, according to the direction and strength of the cable-current. After the middle of February, when the fault was two months old, my method always gave 520 units, and for some days before the cable was repaired, in March, 534 units; the tests after the ordinary method varying as before.

I took some tests in March to localize the fault, and obtained amongst others the following data, which I give as a fair specimen of the others:—

(1.) When distant and insulated, resistance		= 534	units.
Ditto put to earth	ditto	= 530	,,
When line in good order	ditto	= 920	,,

Then by the well-known formula:

$530 - \sqrt{(534 - 530) \times (920 - 530)} = 490{\cdot}5 =$ resistance to the fault. As I have said, the fault was found 493 units distant.

In practice I have found that when the cable-current is positive it is easily eliminated by a negative current, but that when it is negative the operation with a positive current is more difficult. Indeed I prefer not to employ a positive testing current at all, except for a moment when it is required to eliminate a weak negative cable-current. A positive current applied for a few seconds in this manner has only time to depolarise a fault, but when continued longer it seems to actually coat the exposed wire with badly-conducting substances, by which the total resistance is increased.* To illustrate this. Wishing to eliminate a negative current, I put sixty cells positive to the cable for twenty minutes; on putting the line to earth through the galvanometer the needle indicated a slightly positive current from the fault. The needle slowly returned to zero and remained there for several minutes.

* See Appendix B.

Whilst the cable was thus free a test by my method gave 548 units as the "insulation" resistance instead of 507, the resistance before applying the positive current. On several other occasions I reduced the negative current to zero by applying sixty cells positive for ten and fifteen minutes, but the resistance for a long time afterwards varied from 570 to 700 units instead of being 507 or thereabouts.

I have noticed that when the fault is depolarised by a positive current of any duration it does not recover itself for a long time. If a galvanometer be joined in circuit, its needle will remain at or near zero for a considerable time, occasionally oscillating feebly. Thus, fifteen minutes after eliminating the cable-current, the needle was still at zero, and then went over to the positive side, the deflection increasing with slight oscillations. The depolarisation by a negative current on the other hand lasts only for a few moments.

In conclusion of this portion of my paper I feel it necessary to mention that the whole of the foregoing observations do not appear to be applicable to every fault. Thus I have noticed that when the fault has considerable resistance in itself, or when more faults than one exist, it is not always possible to eliminate the cable-current. Again, as I have before remarked, when the fault possesses resistance, the direction and strength of the cable-current, when the distant end is alternately insulated and put to earth, do not always coincide. For example, a fault occurred on a six-mile piece of shore-end cable, which reduced the insulation resistance to about 2,000 units absolute. Now, when the further end of this piece was to earth, I often got (say) a strong negative current, but when it was insulated the cable-current was slight and positive. Again, when the fault is further off than about 150 miles, and the intervening cable perfect, the charge-current interferes with the test.

These exceptions are still engaging my attention, and I mention them here in the hope that some electrician at home, with better opportunities than I possess, may also take them in hand.

In the course of my numerous and varied experiments on the faulty Persian Gulf cable it occurred to me to apply the test with different battery powers, which Mr. Clark recommends as

affording a valuable means of obtaining the approximate resistance of a fault. Mr. Clark says, "The resistance of a *small* fault is much greater with a small battery power, say five cells, than with a higher power, say fifty cells; but if the fault be a *large* one the resistance will be more nearly equal, and if a greater length of copper be exposed the resistances will be the same." In order to demonstrate this I arranged an experimental fault, exposing one inch of copper wire in a bucket of sea water; testing by my method, *i.e.* after eliminating the cable-current, I got 37 units as the resistance of the wire to and including the fault. An ordinary test with 60 cells gave 62 units, and with 10 cells 110 units. With another artificial fault, exposing only a section of small copper wire, the results were:

By my method, ...		resistance	85 units.
By ordinary method, with	60 cells,	,,	110 ,,
,,	10 ,,	,,	160 ,,

So far my experiments were apparently corroborative of Mr. Clark's statements, but, while repeating the tests with 10 cells and with 60 cells alternately, I obtained to my surprise on one occasion nearly the same resistance with the two battery powers. Feeling that I was on the eve of some important discovery I kept on repeating the tests alternately, and soon discovered that when there was no cable-current, and when the battery power was altered rapidly from 10 to 60 cells, and *vice versâ*, the resistance with both powers was exactly the same, viz. 50 units in the first artificial fault and 97 units in the second. From this I concluded that different resistances obtained with different battery powers are due to the presence of a current in the cable, and not, as Mr. Clark says, to the great or small resistance of the fault itself.

My subsequent experiments on the faulty cable, so often referred to in this paper, and on other artificial faults, fully confirmed this view, and enabled me to establish the following points:

1st. When there is no cable-current an ordinary test always gives the *same* result with 10 and 60 cells (or even 10 and 1000 cells). Thus on different occasions I obtained on the faulty cable—

With 60 cells	565	units.	With 10 cells	565	units.
,,	550	,,	,,	550	,,
,,	540	,,	,,	540	,,
,,	575	,,	,,	575	,,

2nd. When the cable-current is negative, and testing with a negative current, the resistance with 60 cells is always *less* than with 10 cells, and for this reason the negative cable-current, by opposing the negative testing-current, makes the resistance greater than it really is; but, as 60 cells are able to depolarise the fault to a greater extent than 10 cells, the negative cable-current (and consequently the resistance it opposes) is less strong in the former case than in the latter.

The following examples taken at random from a large number of observations will illustrate this law. With

60 cells negative resistance	570 units.	With 10 cells resistance	630
,,	610	,,	710
,,	590	,,	670
,,	705	,,	915
,,	640	,,	660

3rd. When the cable-current is positive, and still testing in the ordinary way with a negative current, the resistance with 60 cells is always *greater* than with 10 cells. The reason for which is, a positive cable-current assists the negative testing current and makes the resistance of the wire appear less than it actually is. Now 60 cells neutralize this help (by depolarising it) to a greater extent than 10 cells; and, therefore, as the assisting cable-current becomes less, the resistance apparently increases.

Owing to the rapid polarisation by the negative testing current, I was for some time unable, with experimental faults, to show the effects of a positive cable-current on the resistance with 10 and 60 cells, for although the current, from the fault, was always positive, yet the negative testing current appeared so speedily to depolarise and repolarise it in the reverse direction, that an instant after,

putting on the negative testing current, the current from the fault was really negative too. Again, after removing the battery-current, the fault immediately recovered itself, and gave off a positive current as at first. For example, the cable-current was positive. I tested with 60 and 10 cells negative, and got 115 and 165 units; but, on again observing the direction of the cable-current, I found it was still positive instead of negative.

Suspecting that this was due to rapid polarisation and depolarisation, I took a test quickly, first using 10 cells, then 60, and on observing the direction of the cable-current now found it for an instant on the negative side of the galvanometer; with one or two swings the needle went over again to the positive side. In other experiments with these artificial faults I began by putting 60 cells positive on for some seconds. In this way I succeeded in impressing on the fault a polarisation which lasted sufficiently long to show that 60 cells always gave a higher resistance than 10 cells.

The following were obtained on the cable—

With 60 cells resistance	480.	With 10 cells resistance	420
,,	500	,,	460
,,	450	,,	400
,,	555	,,	520

4th. Varying resistances with the same battery-power are due to varying strengths of the cable-current.

5th. Differences with 60 and 10 cells are due to the same cause. Thus, when the cable-current is strong and negative, the tests with 60 and 10 cells negative differ more than when the current is moderate; when the current is slight the difference is still less, and when there is no cable-current the two tests agree.

APPENDIX (A).

Jask-Henjam Cable faulty. January 17th, 1873.

Observed following Deflections from Cable-current with distant end insulated.

Time.	Deflection.	Remarks.
Midnight.	48°	Positive cable-current.
12·10 a.m.	32°	ditto.
„ 20 „	24°	ditto.
„ 30 „	Zero.	
„ 35 „	4°	Negative cable-current.
„ 40 „	Zero.	
„ 45 „	12°	Positive cable-currents, slight oscillations.
„ 49 „	Zero.	ditto ditto.
„ 50 „	3°	Negative ditto ditto.
„ 54 „	Zero.	Having risen to 6° positive.
1· 0 „	15°	Positive cable-current, oscillations.
„ 5 „	35°	ditto steadier.
„ 10 „	45°	ditto
„ 15 „	50°	ditto
„ 20 „	51°	ditto slight oscillations.
„ 25 „	49°	ditto ditto.
„ 30 „	48°	ditto ditto.
„ 35 „	50°	ditto ditto.
„ 40 „	52°	ditto steady.
„ 45 „	55°	ditto ditto.
„ 50 „	55°	ditto ditto.
„ 55 „	54°	ditto ditto.
2· 0 „	54°	ditto ditto.

Jask-Henjam Cable. January 8th, 1873.

Observed Cable-current on the Mirror Galvanometer as follows—Distant end of Line insulated.

Time.	Deflection.	Remarks.
10 p.m.	160°	Cable-current negative.
„ 5 „	150°	ditto.
„ 10 „	140°	ditto.
„ 15 „	135°	ditto.
„ 20 „	130°	ditto.
„ 25 „	145°	ditto.
„ 30 „	150°	ditto.
„ 35 „	150°	ditto.
„ 40 „	142°	ditto.
„ 45 „	130°	ditto.
„ 50 „	120°	ditto.
„ 55 „	124°	ditto.
11 p.m.	126°	ditto.
„ 5 „	130°	ditto.
„ 10 „	132°	ditto.
„ 15 „	135°	ditto.
„ 20 „	135°	ditto.
„ 25 „	135°	ditto.

All these readings were pretty steady; at times slight oscillation.

Jask-Henjam Cable. January 5th, 1873.
Distant end of Cable to Earth.

Time.	Deflection.	Remarks.
1·0 p.m.	27°	Negative cable-current.
„ 5 „	Zero.	
„ 10 „	17°	Positive ditto.
„ 12 „	24°	Negative ditto.
„ 15 „	38°	ditto ditto.
„ 20 „	33°	ditto ditto.
„ 25 „	26°	ditto ditto.
„ 30 „	45°	ditto ditto.
„ 35 „	54°	ditto ditto.
„ 40 „	60°	ditto ditto.
„ 50 „	65°	ditto ditto.
„ 55 „	54°	ditto ditto.
2·0 p.m.	38°	ditto ditto.
„ 2 „	Zero.	
„ 5 „	47°	Positive ditto.
„ 10 „	58°	ditto ditto.
„ 15 „	64°	ditto ditto.
„ 20 „	64°	ditto ditto.
„ 25 „	62°	ditto ditto.
„ 30 „	53°	ditto ditto.
„ 35 „	35°	ditto ditto.
„ 37 „	Zero.	
„ 40 „	54°	Negative ditto.

Observe how rapidly cable-current changes from positive to negative here.

Jask-Henjam Cable. January 28th, 1873.

Distant end insulated.

Time.	Deflection.	Remarks.	
4·0 p.m.	28°	Positive	Cable-current.
„ 5 „	39°	ditto	ditto.
„ 10 „	35°	ditto	ditto.
„ 20 „	29°	ditto	ditto.
„ 25 „	2°	ditto	ditto.
„ 30 „	25°	Negative	ditto.
„ 35 „	43°	ditto	ditto.
„ 40 „	36°	ditto	ditto.
„ 55 „	40°	ditto	ditto.
5·5 p.m.	27°	ditto	ditto.
„ 15 „	5°	ditto	ditto.
„ 25 „	10°	Positive	ditto.
„ 35 „	10°	ditto	ditto.
„ 40 „	20°	ditto	ditto.
„ 50 „	17°	ditto	ditto.
6·0 p.m.	28°	ditto	ditto.

Jask-Henjam Cable. February 7th, 1873.

Put 60 Cells positive to line through Galvanometer shunted. Distant end Insulated.

Time.	Deflections.	Remarks.
After 1st minute	70°	Oscillations throughout, sometimes violent but generally moderate.
„ 5th „	65°	
„ 10th „	62°	
„ 15th „	61°	
„ 20th „	60°	
„ 35th „	58°	
„ 45th „	57°	
„ 55th „	56°	
„ 70th „	54°	
„ 75th „	54°	

Now put line to earth through the galvanometer, and—

1	minute after	cable-current was	64°	positive
5	do.	do.	64°	do.
10	do.	do.	67°	do.
15	do.	do.	67°	do.

I now took an ordinary resistance-test with the bridge, using O cells negative; immediately after battery-contact the resistance was over 1,000 units, but fell gradually in the course of 35 minutes o 375 units, and then began to rise again.

Jask-Bushire (through Henjam). Jan. 30th, 1873.

Line to earth at Bushire. Tested in the ordinary way, using 60 Cells positive.

Time.	Resistance.	Remarks.
1st min.	536 units	Oscillations.
10th "	600 "	ditto strong.
20th "	690 "	ditto ditto
30th "	760 "	Violent oscillations throughout.
40th "	840 "	
50th "	960 "	
60th "	1120 "	
70th "	1230 "	
80th "	1390 "	
90th "	1440 "	
100th "	1570 "	
110th "	1650 "	
120th "	1500 "	

Now rapidly changed current and tested with 10 cells negative.

Time.	Resistance.	Remarks.
1st min.	1690 units	Oscillations.
5th "	720 "	ditto slight.
10th "	575 "	ditto occasionally.
15th "	545 "	All perfectly steady.
20th "	535 "	
30th "	525 "	
40th "	533 "	
50th "	525 "	
60th "	509 "	Resistance still falling.

Jask-Henjam Cable. Insulated.

Put 60 cells negative on for one hour and a half through a galvanometer. Deflection perfectly steady at 55° during the whole time. Now threw off the battery and observed the cable-current: 1st minute it was 30° negative, 5th minute 50°, 10th minute 60°, 15th minute 62°, and 20th minute 65°—all negative.

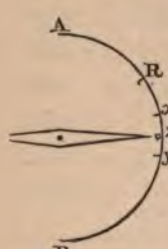
Another time tested with 60 cells negative. Cable to earth at Henjam.

Time.	Resistance.	Remarks.
1st min.	555 units	All very steady.
3rd „	565 „	
5th „	609 „	
7th „	598 „	
9th „	589 „	
11th „	584 „	
13th „	579 „	
15th „	572 „	
20th „	570 „	
30th „	572 „	

DISCUSSION.

Mr. Alexander Adams: During a course of experiments extending over eight months I could get no satisfactory result by eliminating the current of the cable to find out the actual distance to the fault. I tried the Lumsden polar test, and, following up that method experimentally, I came to the conclusion—first, that a zero point of polarity does occur and it will last from two to three seconds; second, that the time occupied in depolarising a fault is proportionate to the time occupied in polarising the exposed end with a given power; third, that the time occupied in depolarising

the exposed end is proportionate to the area of the surface exposed, the time of polarising and power being equal in each case. The last, I think, is a most important result, and may probably account for some of the discrepancies of that test, according to which the approximate amount of surface exposed is obtained, viz., the differences of resistance by 10 cells and by 60 cells. For instance, if you have a cable broken which contains three or four conductors you clean the end by a negative current. I generally use 10 cells, and keep the current up for 10 minutes or a quarter of an hour; then I polarise with a positive current from a testing battery of 40 or 60 cells for one minute. I am careful to mention the time, because unless your watch be in your hand you are likely to lose the result. Supposing I polarise the fault in one conductor for one minute and then reverse my battery to the negative pole, I note the time



occupied in depolarisation and go through the other three conductors, and I find that conductor which, with the same time and power, occupies the greatest time in depolarising, and has the largest surface exposed. I think that is important, because it enables an electrician to choose a conductor for his test. As regards the testing of the fault for distance, I should like to explain my views on the board. Supposing that A B be the degrees of an ordinary differential galvanometer (and I always find a differential is best for working in this way), then z is zero; call A 60° and B 60°. Supposing you have experience in this system of testing, you gradually get your reading in this way: You send a 10-cell negative current for 10 minutes or a quarter of an hour; then, with your watch at hand, reverse to the positive poles of your testing current, and after a minute, or even 30 or 40 seconds, reverse again to the negative pole, and your needle, which by previous testing you have brought to move within readable limits, will leave A and go over the graduated scale until it makes a slight—a very slight—pause at say R; it will then move on again and make a pause of two or three seconds at say x. The needle will then go on again slowly and make a pause at say

after which it will go across very quickly to B, and no resistance will prevent it. Where greatest care has been exercised as regards time of polarising, I have found that the distances R x and y x are nearly equal. I have also found that if you get the point x over the zero of your graduated scale capital results are obtainable from it. Now, upon looking at the cable end through a magnifying glass, as we follow the needle over the scale, we find that after cleaning the end of the cable with a negative current, and then sending a positive current into the cable, we gradually get a white substance formed round the exposed end. We keep the positive pole of the battery on for a minute, then we reverse to the negative pole of the battery, and gradually we find the needle moves over to R. If you look at it through a microscope you find the white chloride of copper has cracked, and falls from the cable end bit by bit, and as it falls the needle gradually moves to x; and just as the needle stops at x you find that the copper is quite clean. This is its zero point, and it can be tried for distance. Directly a bubble rises from the exposed end you find the needle begins moving towards y, and when the bubbles of gas completely envelope the end you find the needle starts quickly from y and falls at once to B. I could not find why, but I proved that by over-coating the end with chloride of copper—by over-doing it—the chloride would crack before I was ready and spoil my test. The great thing is to keep the chloride of copper intact round the exposed end until you bring the negative current to bear upon it. The negative testing current produces what I suppose to be hydrochloride of sodium. It took me eight months to find this, but in five or six cases this test gave good results.

Mr. S. Phillips, Jun.: I am very glad, as a member of this Society, that this important question has been started, if only it will enable us to have the advantage of hearing some remarks upon it from Mr. Latimer Clark and other gentlemen present. I have no doubt we shall all be much obliged to Mr. Fahie, but, with reference to the paper itself, I do not think the author has brought forward anything new or particularly instructive. In fact he appears to me to have gone to some trouble to develope facts which

I presume are pretty well known and generally appreciated at all events by the members of this Society. For instance, he takes pains to prove that the current from a faulty cable is not always positive and steady. I find a statement to that effect in Culley's Handbook of Telegraphy; but he speaks there of the currents coming from the fault pure and simple, and does not mix them up with earth-currents in any way whatever; but one would scarcely be justified from this in going to the end of a faulty cable and always expecting a positive current to be flaming out. Mr. Fahie rather seems to have mixed this matter up, and, judging from the context, does not appreciate the fact that currents coming from a faulty cable would in most cases be due to two causes. First—Earth-current due to a difference of potential between the two points on the earth's surface, and, secondly—The current due to the electromotive force of the fault itself. These two forces may either coincide or oppose each other. Mr. Fahie then comes to the conclusion that it is necessary to eliminate the cable-current. This is in itself a wise conclusion, but I fancy everyone had long ago come to that opinion; in fact, all the tests that exist endeavour to do this more or less, and the real question seems to be—does Mr. Fahie do this better than has been done? In Mr. Culley's book there is a very simple statement as to the mode of testing a fault. He says it should first be polarized with a copper-current, which, we will assume, produces a deflection to the light when the cable is joined through a galvanometer to earth; then at short intervals apply a zinc-current; after each of which the cable-current, due to the original polarization set up by the copper-current, will become less and less, until the battery at the fault becomes inert, and at that moment the measurement is made. Now, quite disregarding other and later methods, what more than this does Mr. Fahie do? Mr. Culley certainly does not tell you to put in another galvanometer, probably because he could as easily see if the line was neutral by the one in use. Neither does he tell you to depend upon the first rush of the current. Mr. Fahie probably had about 76 miles of cable between him and the fault; and, when we consider the large inductive capacity of so long a

circuit, it certainly does seem strange that he should have obtained such good results while adjusting his balance to the first rush of the current. With most galvanometers a violent throw of the needle would have been the result, and the only reason I can suggest why this did not occur is that the inertia of his galvanometer needle must have been considerable; had he worked with a lighter needle I feel sure he would have met with considerable embarrassment, even on a much shorter circuit than 76 miles. Mr. Fahie however speaks of a circuit of 150 miles being likely to cause some trouble in this respect, and says he is investigating the matter, so he will probably enlighten us further on some future occasion; this point however raises the difficulty, that two persons testing the same fault with different galvanometers may from this cause alone get widely different results. We must also recollect that the fault which Mr. Fahie was dealing with was rather a simple case, being evidently a very large one, and one which I imagine would have given results with an ordinary reversal test, taken with care, quite as near as those he obtained.

Mr. J. R. France: I can endorse what Mr. Phillips has said. It seems to me the matter Mr. Fahie has brought before us is such a simple one, that if we had faults of that low resistance I think most electricians would, irrespective of the ordinary mode of testing by the bridge or differentially, use Blavier's method, which would be so certain, or nearly certain, in such a case that it would be scarcely worth while trying any other. It appears, in fact, that he obtained by that formula 490·5 units, whereas the fault was 493 units. I wish we could always arrive at results as close as that. Mr. Adams was speaking of the Lumsden method, which I think most highly of, and I would say, where one has the opportunity of testing a broken cable, the results will be found exceedingly correct, and I think it is a method that ought to be followed as much as possible by electricians. With a multiple cable, my own mode of procedure was always to find a wire that gave the highest resistance. I sealed the end of that wire, and by tolerably close observation I found the inductive capacity. This was about the finest test I could get for multiple cables. In point of fact, in a single wire cable, I should

not dream of testing by the latter method, as the exposed copper is then too valuable to admit of its being destroyed; but, when you have several wires, it is of great advantage to have the induction test to verify the resistance test.

Mr. W. H. Preece: I think the paper just read is a very proper one for this Society; in point of fact it fulfils one of the objects for which the Society was instituted, viz., that those situated in distant places might have an opportunity of laying before their brethren in the profession at home facts that come under their knowledge. We cannot expect everybody to know everything, nor can we imagine a gentleman situated almost at the confines of the earth, as Persia, to be acquainted with all that is being done on our own shores. When we find a young man like Mr. Fahie devoting his time and energy to the promotion of practical questions like this, we ought to hail his paper with great pleasure and give the writer of it all the support we can. The facts he adduces are very few, but they are brought before us clearly. They are not all new as we know, still there are one or two facts that are so. In the conception and drawing up of his paper he has, however, committed an error principally in the use of terms. The author speaks of the different currents sometimes as cable-currents, sometimes as earth-currents, evidently confusing the two. He is not the only one who makes this mistake, and I will try and show you what these different currents really are. Great confusion is introduced by speaking of currents as either positive or negative, and implying thereby two distinct and separate kinds of currents. We must not forget this fact, that there is only one current, which simply varies in direction, and that the terms positive and negative have reference simply to this direction with respect to one position. In one portion of the paper the author mentions, that, to overcome the positive current in the case which he calls the cable current, he applied to the line a positive pole. You would assume from that he was applying the same kind of current, but in point of fact he applies the reverse, that is, a current flowing in a different direction.

Supposing a cable broken and its ends exposed, the current set up is a current formed by elements which consist of the iron sheath

of the cable, the water of the sea, and the copper of the conductor. The copper end is the positive pole, and the current is assumed to flow from this copper pole through the cable to the position of observation where it is said to be a positive current *from* the cable. If now you apply the positive pole of the battery to the cable at the point of observation the current is assumed to flow *into* the cable. Hence it is said to be a positive current *going into* the cable. Therefore, these two so-called positive currents are opposite in kind to each other.

Hence you see how the indiscriminate use of these terms at once leads to confusion. This short preliminary explanation will probably make you comprehend more thoroughly the plan which Mr. Fahie adopted as stated in page 6 of the paper, where he says: "My method, then, consists briefly in first eliminating the cable-current, whether it be positive or negative, and then taking, while the cable is free, a simple resistance test with the bridge in the manner to be presently described. The cable-current is eliminated by sending into the line a current of the opposite sign to that coming from it, and arranging the strength and duration of this current to suit the strength of the one from the cable." Now, there are variations in the character of the currents that are met with in a broken cable, and there are variations in the resistance which the end of the broken cable gives. There is no point connected with the testing of cables which causes Telegraph Engineers so much anxiety and trouble as to account properly for the resistance of the ends. In the first place we meet with earth-currents, those mysterious currents which are due to causes not yet unravelled, currents that vary in direction and strength, and which are very rarely absent from a cable, and which, whenever a cable is tested, must be eliminated or allowed for in some way or other. We have, in the second place, those currents of polarisation which are currents set up by the electrolytic action of the testing current itself at the end of the cable.

In the case of the exposed end of a broken cable, the application of a current to the conductor of the cable decomposes the chloride of sodium, which is the principal constituent of sea-water, and it

thus causes chlorine to be evolved when the current is in one direction, and sodium when in the other direction.

This electrolytic action sets up currents of polarisation at the exposed end which differ in direction to the primary current, and thus disturb its strength. A third cause of disturbance is the cable-current, properly so called, which is set up by the copper of the conductor and the iron of the sheath. We have further the disturbing effect due to the variation of the resistance at the fault, that is, the resistance of the material that surrounds the fault. In the case of a partial fault it would be the resistance of the insulating compound, and in the second place there would be the resistance of the deposit formed by the decomposition of the matter surrounding the fault. Now, these being the causes of disturbance, how are they to be eliminated? It is said that these currents of polarisation, earth-currents and cable-currents, vary the resistance of the fault. But it must be distinctly understood that the variation of resistance is apparent only. These disturbing influences introduce into the measuring apparatus electromotive forces that upset the balance. The balanced currents in the case of the differential galvanometers, for instance, are not equal. The one current going to line is aided or opposed by the other disturbing currents, so that in point of fact the difference of resistance is apparent only, and is simply due to the balance being upset.

Now, how is this to be remedied? The best and simplest way to eliminate the errors introduced by earth-currents or cable-currents is to make the deflection caused by the earth-current the zero of the galvanometer. In the differential galvanometer you have simply to make the deflection produced by earth-currents the zero and then measure up to that. Approximately correct results are obtained also by taking an average of positive and negative measurements. Formulæ are also given by means of which approximately accurate results are easily obtained, so that in case of earth-currents and in case of cable-currents there is little necessity to adopt measures like those adopted by Mr. Fahie. But when we come to the case of currents of polarisation it is a different matter, and unless you have considerable skill and practice it is almost

impossible to get exact results. Of all the methods introduced there is none that I know of that gives more exact results than that which is known as Lumsden's.* It is now based upon a purely scientific fact, and one that with patience and skill must lead to accurate results. It is simply this: when a positive current is sent into the cable the electrolytic action covers the exposed ends with chloride of copper. When a negative current is sent into the cable the exposed end is covered with hydrogen, and the current of polarisation in the first case is the reverse to that set up in the second. If you apply a positive current to the cable you coat the end with chloride of copper, and if then you apply a negative current the chloride of copper is first driven off, and then hydrogen forms itself upon the copper. There is an instant when the exposed end itself is in a clear state; when there is neither chloride of copper nor hydrogen it is in a neutral state, just upon a balance between the two; Mr. Lumsden seizes upon that moment and obtains thereby exact results.

The last method I will refer to is that method of cleaning the end which is described by Mr. Culley in his book, and it is indeed something analogous to that which Mr. Fahie has practically discovered. Mr. Fahie's method appears to be simply this: If there is an earth-current in the cable, he sets up a current of polarization of just the proper strength to destroy it, and thereby to neutralise its disturbing influence.

Currents of polarisation developed themselves as elements of disturbance very early in the days of submarine telegraphy. Mr. Latimer Clark and I spent many days on the rocks of Jersey in 1858 in unravelling their cause and origin, and it is to be regretted that no Society existed in those days to receive the results of our experiments and inquiries.

Mr. Alex. Adams: With regard to Lumsden's test, I have

* This method is fully described in Culley's "Handbook of Practical Telegraphy," 6th edit., sec. 498, *et seq.* It was first applied by Mr. Lumsden in August 1867, but it appears from some correspondence with Mr. Clark that Mr. Laws had previously developed the same method in Aden in 1862, and had constantly used it from that date.

found in my experiments that the point which he takes as zero is not the zero point, but it is past it. You will find there are three apparent zeros, viz., the first R, the centre x, then the next y, and it is the last which Lumsden takes as his zero. I think that will be found to be a mistake.

Professor W. G. ADAMS, F.R.S.: In testing cables by the Wheatstone's bridge method, Mr. Fahie seems to have taken his resistances A and B equal to one another (see fig.), and to have determined his insulation resistances on the supposition that the resistances of the cable and of his resistance R are equal, even when there may be an earth-current or polarization-current on the cable at the time.

This of course will not be true, and hence it is the usual plan to create counter disturbances in the cable to balance the earth-currents, in which case the resistance of cable and fault is equal to the resistance R. When the earth-currents are balanced or balance one another throughout the length of the cable, and not only at the testing end, this will be strictly true; but, seeing that there is great difficulty in bringing the cable to a state of comparative calm, I would ask practical men whether it is not possible to determine the resistance of a cable by measuring the earth-current and comparing it with the current employed to charge the cable.

With Mr. Fahie's arrangement, as shown in the diagram, if Q be the current from the battery into the cable, and q an earth-current into the cable in the same direction, and γ the resistance of the cable, then by Ohm's law

$$\gamma\,(Q + q) = Q\,R, \text{ or } \frac{\gamma}{R} = \frac{Q}{Q + q},$$

q and Q will be directly proportional to the potential at the testing end immediately before and on charging the cable, and so their ratio may be readily measured.

I would also ask whether, in practice, the position and resistance of a fault in a line or cable may not be readily found without employing the Wheatstone's bridge method, by determining the strength of currents or the potential at both ends of the line or cable when a charge is given to it.

According to Ohm's law it should be easy to find how far the fault is from the middle of the line by comparing the effects of the fault on instruments at both ends at the same time. A fault will increase the amount of the charge on a line at the charging end, but the further off a fault is from the charging station the less will be the effect of that fault in increasing the amount of the charge given to the line. The charge produced by a fault on the instruments at the receiving station will be greatest when the fault is in the middle of the line, and when a fault of the same resistance is at equal distances from either end of the line the charges produced by it at the receiving station will be equal to one another. Whatever be the resistance of a fault, if it occur exactly in the middle of the line, the charges at the two ends are altered by the same amount; one is increased by as much as the other is diminished, so that the mean of the two charges at the two ends of the line is equal to the mean of the charges given by the same battery when there is no fault in the line.

If we call Q the charge when the line is perfect, and Q_1 Q_2 the charges at the charging and at the receiving end respectively, r_1 and r_2 the resistances of the two parts of the line, and x the resistance of the fault,

then $$\frac{Q_1}{Q} = \frac{(r_1 + r_2)\,(r_2 + x)}{r_1 r_2 + (r_1 + r_2)\,x} \quad \text{(i.)}$$

and $$\frac{Q_2}{Q} = \frac{(r_1 + r_2)\,x}{r_1 r_2 + (r_1 + r_2)\,x} \quad \text{(ii.)}$$

So that $$\frac{Q_1 + Q_2}{2\,Q} = \frac{1}{2} \times \frac{(r_1 + r_2)\,(r_2 + 2\,x)}{r_1 r_2 + (r_1 + r_2)\,x},$$

which becomes equal to 1 when $r_1 = r_2$, whatever be the value of x.

The values of r_1, r_2, and x are given from these equations when Q, Q_1, and Q_2, and the total resistance of the faultless line are known.

The Chairman: I will first reply to the remarks of Professor Adams as well as I can. The formula which takes into account the effect of the earth-currents at either end of a line is well

known, and is published in electrical works, but it is not commonly used in practice; one of the chief reasons being that it is found impossible to get earth-currents to remain steady for even two or three minutes together.

The other proposition seems to me deficient in not taking account of the varying potential of the fault itself and the currents entering or leaving the line at the fault, the amount of which you do not know, and this complicates the calculation for the position of the fault. But the plan of measuring the position of a fault by the potential at the two ends has been broached both by Sir William Thomson and myself, and, by using batteries of absolute uniformity of potential, he believes he is able to make an acurate test of the position of the fault.

I now come to the paper. The subject brought before us in the paper to-night I think is one of the greatest importance to telegraphy. We all know and must at once see how important it is in lifting a cable for a fault, to know exactly where the fault is. It is a subject worthy the attention of this meeting, and I hope Mr. Fahie's contribution (which is an excellent one) will induce others to make experiments in the same direction, and will lead to other communications, or to a complete treatise, on the testing of faults in cables, for we have only touched upon the fringe of the subject to-night. I do not know a more pleasing or instructive experiment, than to take a bucket of sea-water, and, having made an artificial fault in a piece of cable by making a clear section of the end of the wire, to send positive and negative currents through the exposed ends of the wire, and to watch under a microscope the changes that take place simultaneously with their measurement by the galvanometer. You see clearly everything that is going on. By putting on a positive current, you coat the copper with a grey or dark sub-chloride of copper, and you increase the resistance by polarising the fault. In this state the fault tends to return a strong positive current in opposition to your own. By reversing the current you clear off the deposit from the copper, and you see the bright metallic copper appear, and at this instant, when the copper becomes clean, the polarisation becomes zero, and the resistance is

at the lowest, and you get the nearest measure of the true position of the fault. After this the resistance again increases, and the polarisation is reversed. This is a very instructive experiment, which every student of submarine telegraphy should often witness for himself. There is an intermediate moment when the copper is bright and clean, and when the resistance is at the lowest point, and the fixing of this point is what is called Lumsden's method, and it is one of the most useful ways of determining the real resistance of the conductor and fault. I am sorry Mr. Laws is not here to-night, for he could not only have given us some valuable information on the subject, but he might have thrown some light on its history. I confess it surprised me to hear that method described as Mr. Lumsden's. I do not know when it was introduced by him, but in April, 1862, I was in the Indian Ocean with Mr. Laws, and we certainly tested for faults by reversing the current, and finding out the point of least resistance, both practically at sea, and in the testing-room with a microscope, as I have just described.

Mr. Fahie's plan and principle appear to my mind good. I think his system is a more scientific one than Mr. Laws' or Mr. Lumsden's, and is a distinct improvement on them. He, as Mr. Preece described it, actually brings the cable for a moment into a neutral state, and must therefore get the resistance of the cable and fault more accurately than by any other method. Mr. Fahie, in his remarks on my method of estimating the resistance of a fault, by using two battery powers of different potentials, points out that the variation is due not to actual differing resistances of the fault, but to changes of polarity; he has in this made a very useful observation, and has, I think, shown the correct cause of the variation. But I still consider that this method is a useful one for determining whether the fault in a cable is a small one or a large one. The error I committed was not in stating the facts but in using the words I did, which were, "the resistance of a small fault is much greater with a low battery power, say of five cells, than with a higher power, say fifty cells; but if the fault be a large one the resistance will be more nearly equal, and, if a very great length of

copper be exposed, the resistance will be the same with both powers."

I ought to have said the *apparent* resistance. I am very much pleased with Mr. Fahie's paper, and, as Mr. Preece truly says, it is a class of communication which we are most anxious to encourage. We are most desirous to obtain both the practical experience and the ideas of persons placed in official positions like Mr. Fahie, and to have the results of their observations brought before us for the benefit of the whole profession. I therefore feel that this meeting ought to record its high sense of the value of this paper by passing a vote of thanks to Mr. Fahie, which I have now great pleasure in proposing.

The Meeting then adjourned.

The Twenty-ninth Ordinary General Meeting was held on Wednesday, the 25th November, 1874, Sir WILLIAM THOMSON, President, in the Chair.

The following paper was read—

EARTH-BORING FOR TELEGRAPH POLES.

By JOHN GAVEY, M.S.T.E.

In the present age, when labour is daily becoming more costly, and labour-saving appliances are gradually superseding mere physical strength in all mechanical operations, it may be well to inquire what improvements have been introduced with the object of reducing the expenditure of manual force in, and the cost of, the erection of telegraph poles.

The old method of digging a post-hole by means of a spade and pickaxe is so well known as to need but a very brief description.

A rectangular opening is made in the ground, the length averaging about 4 feet by a width of 2 feet. These dimensions are continued to a depth of some 2·5 feet, whence, by a step-like arrangement, the length of the hole is gradually shortened, until at the bottom it does not exceed one foot. This is illustrated in the accompanying sketch. (Fig. 1.)

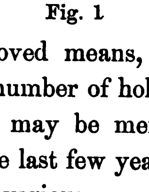


Fig. 1

In comparing this system with new and improved means, it becomes necessary to consider what is the average number of holes that can be dug by one man per day. And here it may be mentioned that the enormous impetus that has within the last few years been given to telegraphy in its broadest sense, by various causes which it is unnecessary to consider at present, has caused the average dimensions of telegraph poles to be considerably increased; the depth of the pole-holes and consequent cost of erection being of course correspondingly greater.

Some years ago a pole 24 feet long, by 5 inches diameter at the

top, was considered a fair length for a main line. Now, lengths of 28 and 30 feet, with a minimum diameter of 6 inches at the top, are as a rule adopted.

The depth at which it is customary to plant telegraph poles may roughly be considered to vary from 4 feet 6 inches for a pole 24 feet long, to 5 feet 6 inches for one 28 or 30 feet; 6 feet serving up to, say, 40 feet, and 7 feet for any height.

Now, the number of pole-holes which an ordinary labourer, accustomed to the work, will dig in a working day of ten hours will obviously depend on the depth which he has to reach and the soil in which the poles have to be planted. We may assume, as before indicated, that the average depths will vary from 4 feet 6 inches to 5 feet 6 inches, and the soils may conveniently be divided into—1st, clay; 2nd, ordinary soil, requiring some use of the pickaxe; 3rd, hard gravel; 4th, chalk; and 5th, rock.

It may be taken as the result of experience that the average number of holes which a workman will dig in 10 hours is represented in the following table:—

SOIL.	No. of holes per day.	
	4 feet 6 inches deep.	5 feet 6 inches deep.
Clay	6	5
Digging Soil . . .	5	4
Hard Gravel	4	3 to 3½

Chalk and rock may for the present be left out of consideration. For these there appears no alternative but the pick-axe, bar, spade, and, occasionally, blasting.

The digging of a pole-hole 4 feet 6 inches deep may be said to involve the removal of 30 cubic feet of soil, which will represent a weight varying from 2,850 lbs. to 3,600 lbs. according to the nature of the soil; whilst one 5 feet 6 inches deep will contain 44 cubic feet, with a weight of from 4,180 lbs. to 5,280 lbs. This allows for the step-like manner in which such a hole is generally dug.

It will of course be obvious that the most ready means of diminishing the labour involved by these operations will be the reduction of the size of the hole; for we actually require but a cylindrical space, varying from 10 to 14 inches in diameter, with an average cubical content of 3·5 feet, and a weight of materials of 376 lbs. for a 4 feet 6 inch hole; the contents of one 5 feet 6 inches deep being taken at 4·3 cubic feet, weighing 460 lbs.

Perhaps the earliest attempt in this direction was made in Spain, where an instrument since known as a "Spanish spoon" was introduced. This was reported to have worked very successfully. It may be said to have consisted of a section, or arc, of a circular metal disc; the chord of the arc forming a cutting edge. The periphery of the disc was provided with a ledge about 2 inches high, to retain whatever might accumulate on its surface, and the whole was fixed at right angles to a long handle. To dig a hole with this machine, the soil was first loosened by means of a long bar, and, the machine being inserted, a circular or rotary motion was imparted to it, by means of its handle, whereby the loosened soil was accumulated on its surface and removed from the hole.

A similar instrument was tried in this country, but the results obtained were not so satisfactory as was expected.

The average speed with which a pole-hole, varying from 4 feet to 4 feet 6 inches, was dug, did not exceed that of the ordinary method; but of course an advantage was presented by the new system in the additional solidity of the pole when erected, and in the diminution of the quantity of soil which had to be filled and rammed in. It was observed that after a depth of about 3 feet 6 inches had been reached the difficulty of loosening and collecting the soil appeared to increase, and the speed of working to decrease, as the square of the additional depth, so that the plan did not at that time promise much success with deeper holes for heavy timber.

Various modifications in the form of the spoon have been introduced with more or less success; but the principle of all forms of this apparatus is the same, namely, loosening the soil with a suitable bar, and collecting and raising to the surface the débris thus broken away.

The most marked improvement in the method of opening holes for telegraph poles appears to have been the introduction of earth-borers, amongst which may be mentioned those invented by Spiller, by Marshall, and by Bohlken.

In most soils these instruments obviate the necessity of breaking up and loosening the materials with a bar, and moreover they collect the débris with much greater facility than is possible with a Spanish spoon.

Spiller's borers may be described as a huge modification of the old-fashioned ship's auger. It consists of a plate of sheet iron, bent into the form of a semi-cylinder, attached vertically to a long handle. At the lower extremity, and at right angles to it, are two quadrants of sheet-steel, one slightly bent downwards, which forms the cutting plate; the other, which occupies the second half of the semi-cylinder, is bent upwards, and serves to retain the débris as it is cut up by the lower plate.

Marshall's borer is thus described in his specification:—

"The auger blade is made of a flat plate or disc of metal, severed in a radial direction from the centre or boss, on which the blade is carried, to the circumference, the two ends of the blade thus formed being bent apart of a V form in edge view. The edge or point which is bent downwards or projects from the lower surface of the blade forms the cutting edge, the earth removed thereby on the rotation of the auger passing through the radial opening between the two ends on to the upper surface of the plate, whence it is removed from time to time by raising the auger. The auger or blade is fixed at right angles on its shank or stock, which is formed with a squared end to receive the boss of the blade.

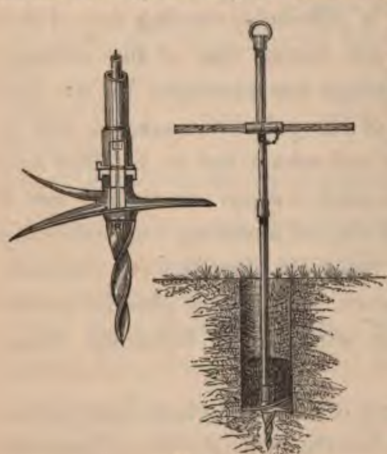


Fig. 2.

The latter is secured by a tapering metal point screwed on to the extremity of the stock. The point is provided with a quick

spiral groove or grooves, to form a rimer for the purpose of drilling a hole in advance of the boss of the auger, and so facilitate the penetration of the latter. The stock of the auger is attached to two or more sections of tubes screwed together and provided with sockets for the cross handle used for rotating the auger." (See fig. 2).

Bohlken's borer is an instrument very similar to that introduced by Marshall. The principle appears to be exactly the same.

These borers should invariably be accompanied by a narrow long-handled shovel and a punner bar, so called. (Figs. 3 and 4). This bar consist of a long hollow rod, at one end of which is a chisel point for loosening the soil, breaking and removing stones, &c., and at the other a peculiarly-shaped punner head for ramming and consolidating the soil around the pole when this is erected. The shovel is found to be of much service in removing stones and other obstructions which interfere with the passage of the auger. These augers are worked by two men, who rotate them steadily, withdrawing them from time to time with their superincumbent load of soil.

Fig. 3. Fig. 4.

Spiller's borer was found by the author to be very effective in sand, clay, and ordinary soil. It, however, was not so successful in gravel, as it would not bore through it. The whole of the material had to be disturbed and broken with a bar, the borer then simply acting as a spoon, and a somewhat clumsy one, to remove the débris. Moreover the radial opening between the plates appeared too small to admit of the passage of ordinary gravel and stones. It is, however, probable that the instrument might be so modified as to give better results with the last-mentioned soil. The machine itself is very strong, will bear much rough usage, and does not appear likely to break or get damaged by hard work.

Marshall's borer is very effective in clay, ordinary soil, and gravel. It does not simply bore into the ground like a common screw pile, for then it would be impossible to withdraw it. The action of the cutter takes slice after slice of the soil, raising each

through the height of the groove, thereby severing it from the circumference of the hole, without otherwise disturbing its position. A small valve exists in the tapering point of the machine, which is moveable by means of a lever or eccentric and a connecting-rod which passes down the hollow stem. The object of this is to admit of the introduction of air under the plate in such soils as would otherwise tend to form a vacuum on the withdrawal of the closely-packed borer, thereby adding the weight of the atmosphere to that of the materials raised.

The cutting-plates, which are interchangeable, vary from 10 to 14 inches in diameter. These sizes will, as a rule, serve for the majority of poles erected; but the hole can, if necessary, be easily enlarged with a properly-formed shovel, by cutting down the sides and raising the soil with a borer.

With this machine a hole has been bored a depth of 5 feet 6 inches, in clay, by two men, in twenty minutes, and the pole erected and completed in half an hour; and holes in gravel have been opened up a like depth, in a time varying from thirty minutes to an hour, with the same strength.

There is obviously a very considerable saving of labour in filling in and ramming the bored hole, as compared with that dug in the ordinary manner. It may be mentioned that in boring through gravel it is found necessary to drive the bar down the centre of the hole, well in advance of the borer, to admit of the entrance of the tapering point which forms the extremity, in order to get through the gravel at a fair speed. The ordinary rotating motion does the remainder of the work.

Bohlken's borers appear the same in principle as Marshall's, and would probably do like work. The author has not used them.

These borers have obviously one defect. They cannot be worked close to a wall or a hedge, positions where, in road telegraphy especially, it is frequently necessary to erect poles. This difficulty has been overcome by fitting one of the borers with an ordinary ratchet handle, lightening such of the parts as admit of it, and replacing the long tapering point by a shorter one. This modified instrument will bore in clay and ordinary soil, but in gravel it only acts as a spoon, the soil having to be broken up with a bar.

The advantages of these apparatus may be summarised as follows:—

1st. *Speed.*—A number of pole-holes can be opened out to a given depth, with the same force of men, in less time, than by ordinary methods.

2ndly. Diminution of labour in filling in.

3rdly. Greater solidity of the pole thus erected.

4thly. Less disturbance and mixing up of the soil,—a point of some consequence in erecting lines of telegraph through private property.

5thly. The advantage of being able to work in wet soils without baling or pumping.

6thly. Saving of expense in sharpening and laying pickaxes.

Amongst the disadvantages may be mentioned:—

1st. Strain involved in raising the borers out of heavy soils, with their load of débris.

2ndly. The difficulty, and with inexperienced men the danger, in rearing the pole into the hole, when the former is very heavy.

3rdly. Liability of the screw-borers to fracture, and additional first cost.

A few words on these points may be desirable.

Advantages.

1st. *Speed.*—There is no doubt that under all ordinary conditions, and even with men who have acquired but slight skill in the use of the apparatus, a greater number of holes can be bored in a given time, with the same force, than could be dug by ordinary means; and this in all classes of soils that admit of boring operations. Even large stones do not interfere to any extent with the boring, unless of course the soil consists of rock, for in the former case these obstructions are broken up with a bar and removed, by hand, with the borer, or with the shovel.

2ndly. *Diminution of Labour in Filling-in.*—This is so obvious that it is not necessary to dwell on the point.

3rdly. *Greater Solidity of the Pole when Erect.*—This is, perhaps, one of the greatest advantages obtained by the use of borers. Where

a large rectangular hole is dug out, and the soil is simply filled in and rammed, even when every care is exercised, and a strict watch kept on the men, it is certain that the soil cannot be rendered so firm and solid as it was before its disturbance; and a period of two or three years sometimes elapses before this state of solidity is again reached. In the meantime every gale that blows causes the poles to sway in their loose beds, and this adds enormously both to the liability to faults and to the strain exerted on the timber itself. Moreover, in the latter case, there is always a temptation for the foreman, or, in his absence, for his men to get over their work quickly, by simply heaping in the soil without proper consolidation. With a bored hole, a firm, hard, cylindrical, undisturbed surface is presented, slightly larger in diameter than the pole itself, and, the space between the two being small, it offers little inducement for the men to omit the ramming; and further, this space being inclosed by two solid surfaces, the soil can be so firmly consolidated that movement of the timber is almost impossible. It is rarely, if ever, found necessary in bored holes to use blocks of timber to resist the lateral pressure on the soil. An additional saving both in materials and labour is hereby afforded.

4thly. *Lesser Disturbance of the Soil.*—This is an undoubted advantage in road telegraphy. Few landowners or proprietors care to see huge holes dug in their fields, gardens, and hedges; but, when it is only necessary to bore a cylindrical hole 10 to 14 inches in diameter, much less objection is felt to the operation, and much less damage is done to existing vegetation.

5thly. *Advantages in Wet Soils.*—The delay, annoyance, and discomfort to the workmen, caused by a flow of water into a pole-hole under the old method of digging, is well known. With the one exception of running soils, there is no additional difficulty caused by water in boring operations, which are carried on under these circumstances with the same facility as in dry soils.

6thly. *Saving in Repairs to Tools.*—This item is probably balanced by the fracture of boring-plates and repairs to apparatus referred to below.

Disadvantages.

1st. *Strain involved in raising Borers.*—It is frequently found that some difficulty is experienced in raising the borers out of the ground when too heavily laden. This is due to the friction between the mass of soil and the circumference of the hole. This difficulty is more obvious with the screw-borers than with Spiller's, but it is found that when the borer is not overladen it can generally be raised without unfairly taxing the men's strength. The amount of soil which can fairly be removed without overstrain is soon arrived at by experience, and the men themselves can be trusted to correct an error of this kind. Another means of overcoming the difficulty is dwelt on below.

2ndly. *Difficulty in raising heavy Poles.*—There is naturally greater difficulty in rearing poles into a bored than into a dug hole, as in the former case the dead load of the pole has to be supported by the men until the latter is raised into an almost perpendicular position, and, moreover, an injudicious application of strength on one side, when the pole is, say, at an angle of 45°, will cause it to sway right over and fall to the ground. With a rectangular hole the sides and ends of the latter support and steady the pole, and admit of a series of efforts, followed by a series of short rests, on the part of the men. Of course with light timber this is a matter of comparative indifference, but the aspect of the question is altered in dealing with heavy poles. These require a certain amount of skill and confidence to raise them satisfactorily.

The ordinary method of rearing poles has been used for bored holes, the punner-bar being placed vertically in the hole for the butt to rest against as a fulcrum, and props, or lifting-rods, being supplied to raise the head of the pole in the well-known manner. This is amply sufficient for poles not exceeding 24 or 26 feet long. A method of dealing with heavier timber is referred to further on.

3rdly. *Liability to Fracture.*—This has only been observed up to

the present time in gravel, and it may probably be obviated by the introduction of a tougher material for the borer-plates.

Marshall's plates have hitherto been made of cast-iron. It is probable that the use of malleable cast-iron, gun-metal, or cast-steel, would effect a great improvement in this respect.

It was thought that the two first disadvantages might be obviated by some simple means, and accordingly light three-legged sheers were prepared. These were made from ordinary pine, free from knots, about 3 inches square, slightly tapering in the ordinary manner, and about 21 feet long. They were hinged at the upper extremity, so as to fold closely, and were easily handled by three or four men, according to their weight. A pair of blocks and tackle, together with a small winch, were supplied with each set. The sheers might perhaps be made of hollow iron tubing of a suitable diameter, and they would be found of service in other than boring operations.

In a recent work, two pair of sheers of this description were introduced: one pair was worked by three men—the second, which was slightly heavier, required four. Two gangs with these respective numbers started on a given length. Each raised its own sheers, bored the holes, drawing the borer out by means of the sheers; then, without further change, slung, raised, and set the poles, which ranged in length from 30 or 40 feet, with a minimum diameter of 6 inches at the top, and which moreover were fully impregnated with creosote or sulphate of copper. The soil in which these poles were erected consisted of clay, sand, and hard gravel; and the average duration of each operation, from the time the men finished at one pole till they had completed the following one, was 1 hour 46 minutes. This averages 11 poles per day for the seven men, and, taking into account the size of the timber and the unfavourable character of the ground, speaks favourably for the system.

The like work would scarcely have been done by a similar number of men with pickaxe and spade, whilst for a considerable subsequent period the stability of the line would have been nothing like so great in the latter case as in the former. In this length moreover certain A poles were erected without the cross-ties gene-

rally used at the bases. The saving in labour was enormous compared with the ordinary method, and the poles appear to have stood as well, up to the present time, as those erected in the usual manner with the cross-braces at the butts.

If it be found that sufficient rigidity can be imparted to A poles by setting them in bored holes, a very considerable economy will be effected in erecting heavy lines, and the subject is one which perhaps should be dealt with a little more at large.

An A pole may be described as an arrangement of two poles, devised with the object of giving the greatest possible rigidity where a narrow base is imperative, and without the use of expensive iron ties.

The ordinary form consists of two poles, slightly scarfed at the tops, and fitted together as an isosceles triangle with an iron crosstie about 8 feet from the ground, and a wooden brace at the base of the structure. (Fig. 5.)

Fig. 5.

Now it is obvious that in resisting a lateral strain placed at the apex of the triangle the whole structure acts like a bent lever, the weight being distributed over C B and the fulcrum being placed at B or D; at B in recently disturbed soil, at D if at this point it rests against a solid substance. We may practically consider the fulcrum to be placed at B, and under ordinary circumstances the hold on a recently-disturbed soil would be less than the lateral strength of the structure, and under heavy strains the latter would probably move over bodily. It is, however, generally arranged that the tie C B is partly embedded in solid undisturbed earth, and

we thereby get an amount of rigidity at the base corresponding with the strength of the poles above ground.

With an A pole set in bored holes the circumstances are widely different, only a small portion of soil having to be rammed in, and, this being inclosed between two solid surfaces, there is scarcely any limit to the degree of consolidation of the disturbed earth that can be obtained, except that imposed by the materials themselves.

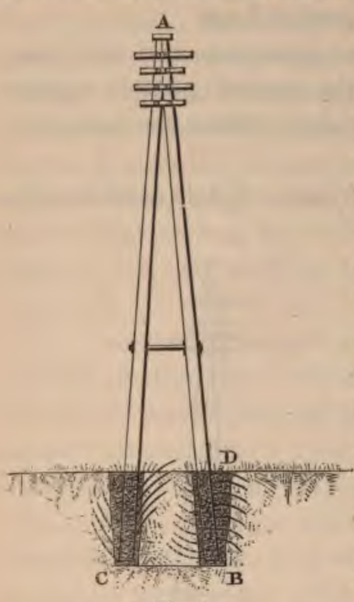


Fig. 6.

To move a pole set in this manner it becomes necessary to displace the solid earth in the directions shown by the curves in fig. 6. In this, if we consider B as the fulcrum, the lines of movement will be represented by the whole curves; if D be sufficiently consolidated to represent the fulcrum, the dotted curves will then represent the lines of movement or of resistance. Practically a combination of these movements or resistances would be brought into play. The sum total of these resistances would be so great as to make it very probable that they would satisfy all ordinary requirements in any but the lightest soils. These of course should be dealt with specially.

The economy resulting from this method is very considerable, as not only would the cross-braces, bolts, and labour of fitting be saved, but the digging of a huge pit, which in all cases assumes dimensions far exceeding those of an ordinary post-hole.

It is not suggested that cross-braces should be wholly abandoned, but it is thought that the system of erecting A poles in bored holes, without ties at the base, is worthy of further consideration and trial in such soils as are sufficiently solid to afford good ground-hold on the structure. The arrangement may in short be considered as an improved method of performing the ordinary operation of strutting.

Since the reading of the foregoing paper and the discussion thereon, certain improvements or modifications have been made in Marshall's borer which tend to overcome some of the objections that existed to the use of the former apparatus.

An improved borer, formed of a broad flat plate twisted into a spiral, somewhat resembling an exaggerated corkscrew, with an increasing pitch, and terminating in a point, has been found very effective, so far as experimental trials have gone. It raises large stones more readily than the original form; it is made of wrought iron, and is not liable to fracture, and it appears well adapted to general work. A hole up to nine or ten inches in diameter can readily be bored by one man, who is not compelled to walk around the machine, but works it standing in one spot. For larger holes two men are still needed; but there is no occasion for their moving round the hole. Hence post-holes can be bored close to walls, hedges, and on sloping embankments with facility; and the duplication of tools for this purpose involved by the original form is avoided.

Further, the abolition of the necessity for the monotonous movements of the men which accompanied the use of the other borers will diminish the objections which labourers had to boring operations.

Two tables are appended, showing some results obtained with borers. The first shows a comparative trial between a "Marshall's" borer, a "Spiller's" borer, and a "Marshall's" modified form for working near walls. The second gives the results obtained in erecting sixty-nine consecutive poles, principally heavy Scotch fir, recently boucherized, of a minimum length of 30 feet and ranging to 40 feet, by seven men with two light sheers. The time quoted is from the completion of one pole to the completion of the next.

It should, moreover, be remarked, in justice to the apparatus, that, with one individual exception, the men were new to its use, and that several minor defects were discovered and remedied, either at the time of trial or later. This naturally caused delay, and rendered the results less striking than they might have been under more favourable circumstances.

Comparative Results with Spiller and Marshall's Borers.

(Spiller's Borer.)

No. of hole.	Time boring.	Depth. Sand and loose soil.	Clay and marl.	Gravel.	Remarks.
	mins.	ft.	ft.	ft.	
2	43	1·0	3·3	1·3	
6	84	2·9	—	2·9	
11	60	2·0	3·6	2·0	
15	93	—	3·0	2·6	
19	86	—	1·0	3·0	After 86 mins. work hole only four feet down. Experiment then ceased.
	366	5·9	10·9	11·6	

(Marshall's 12″ Borer.)

No. of hole.	Time boring	Depth. Sand and loose soil.	Clay and marl.	Gravel.	Remarks.
	mins.	ft.	ft.	ft.	
3	28	—	3·6	2·0	
4	47	3·0	—	2·6	
8	80	—	2·6	3·0	Nos. 7 and 8 bored for A pole, 35 mins. additional taken up for widening both holes.
9	40	0·9	2·9	2·0	
10	58	—	3·6	2·0	Plate broken.
14	63	—	2·9	2·9	Nos. 10, 14, 16, and 18 poles, 3 men with sheers were told off. The sheers erected for drawing borer, and, when hole complete, used for raising pole. Work done entirely by them.
16	63	—	2·9	2·9	
18	87	—	2·0	3·6	
	466	3·9	19·9	20·6	

MARSHALL'S RATCHET LIGHT BORER.

(Spoon Borer.)

No. of hole.	Time boring.	Sand and loose soil.	Clay and marl.	Gravel.	Time raising and punning poles.	Means of raising poles.	Remarks.
	mins.	ft.	ft.	ft.	mins.		
1	35	2·9	2·9	—	8	Hand 8 men	
5	48	3·0	2·6	—	24	Sheers 3 men	
7	104	3·0	—	2·3	For A *vi*	*de* No. 8.	
12	124	—	5·0	·3	15	Hand 8 men	
13	52	4·0	—	1·6	27	Sheers 4 men	
17	88	—	3·6	2·0	21	Sheers 4 men	The ratchet broke early in the boring of No. 12 hole, and the hole was got out with the defective tool.

RESULTS OBTAINED IN THE ERECTION OF SIXTY-NINE POLES WITH "MARSHALL'S" BORER.

Boring apparatus worked with sheer-legs:

3 men with light sheer-legs.

4 men with heavy ,, ,,

Poles boucherised, 30 to 40 feet long.

The time given includes the raising of legs, boring hole, erecting pole, &c., complete.

No. of pole.	Total time.		Description of soil and remarks.
	H.	M.	
94	1	35	All gravel, hard
95	1	41	Sand and gravel
96	1	25	ditto ditto
97	1	55	Gravel, very hard
98	1	40	1 foot of clay and 4 feet 6 of gravel
99	1	35	Gravel, hard
100	1	40	ditto ditto

No. of pole.	Total time.		Description of soil and remarks.
	H.	M.	
101	1	55	All gravel
102	2	0	ditto
103	2	15	Sand and gravel
104	1	30	ditto ditto
105	2	0	Gravel, hard
106	1	58	All gravel, very hard
107	1	48	Gravel, very hard
108	3	25	All gravel, very hard
109	2	15	Gravel
110	1	30	ditto
111	1	50	Sand and gravel
112	1	45	Gravel, very hard
113	2	14	Gravel
114	2	10	ditto
115	2	5	Gravel, very hard
116	2	2	Gravel
117	2	12	Gravel, very hard
118	2	34	Gravel
119	1	50	Sand and clay
120	1	45	ditto ditto
121	1	48	ditto ditto
122	1	45	Sand and gravel
123	2	9	Gravel, very hard
124	2	5	All gravel
125	2	10	Gravel and sand, very hard
126	1	20	Clay and gravel, hard
127	2	14	All gravel, hard
128	1	22	Clay and sand, hard
129	0	58	1 foot of gravel, 4 feet 6 of clay
130	1	10	Clay and sand
131	1	0	ditto ditto
132	2	10	2 feet of gravel, 3 feet 6 of clay and grave
133	2	6	All gravel, very hard
134	4	18	4 feet of gravel, 1 foot 6 of clay and grave (raining 1 hour)

No. of pole.	Total time.		Description of soil and remarks.
	H.	M.	
135	3	50	2 feet of gravel, 3 feet 6 of clay (raining 1 hour)
136	1	13	All gravel and clay, very hard
137	1	25	All gravel and sand
138	1	39	All gravel, very hard
139	1	31	ditto ditto
140	1	30	Sand and gravel
141	2	17	Gravel, very hard
142	1	20	Sand and gravel
143	1	32	ditto ditto
144	1	30	ditto ditto
145	1	45	Sand and gravel, hard
146	1	25	1 foot of sand, 4 feet 6 of gravel
147	2	13	Sand and gravel, hard
148	1	33	Clay and gravel, very hard
149	1	40	Clay and gravel
150	1	25	Sand and gravel
151	0	50	Clay, very hard
152	0	35	Clay
153	1	20	Sand and gravel
154	1	5	ditto ditto
155	1	5	ditto ditto
156	1	5	Clay and sand
157	1	45	Sand and gravel
158	1	25	Clay and sand
159	1	40	All gravel
160	1	55	Gravel
161	1	46	Sand and gravel
162	1	51	ditto ditto

Mr. Gavey gave a description of the various instruments exhibited by which the paper was illustrated, and stated that he had received a communication from Mr. Marshall to the effect that he had recently erected sixty miles of line in Venezuela solely by means of the borer exhibited. With reference to the subject of the A poles, he had received a letter from Mr. Langdon, which he begged permission to read to the Meeting—

Southampton, *Nov. 24th*, 1874.

I have read your paper on "Earth-boring for Telegraph Poles" with very great pleasure. Only those who have seen a hole dug for an A pole can realise fairly the amount of labour involved as compared with that required for boring two holes for what may be termed the legs of an A pole. There is of course the possibility that in some soils the varying strain occasioned by the force of the wind may gradually work the poles out of the ground. Probably the idea has suggested itself to you that this might be in a great measure met by fitting a kind of shoe to the bottom of each pole. If so you will no doubt give the Society the advantage of the suggestion in your concluding remarks.

I would not, in using such an appliance, fix it to the bottom of the pole, but about an inch or so up. It should be fitted with ears, so that it might be bolted on to the pole above the shoe portion, where there would be little fear of its giving way whatever pressure might be brought to bear upon it.

There is this drawback about it. It would necessitate the hole being bored from 1 to 2 inches larger than would otherwise be needed; but still I think it desirable something of the kind should be used. It need not necessarily pass all round the pole; it would, I think, be sufficient if it were merely semicircular.

I am sorry I shall not be present at the reading of the paper, but, should the discussion be carried over, I hope I may be more fortunate for the next Meeting.

Yours very truly,
W. Langdon.

J. Gavey, Esq.

Mr. Gavey remarked he thought there was but little danger of the poles being carried out of the ground, unless the soil were of a very light character. In these cases special methods should of course be adopted, but in more solid soils he recommended bored holes for A poles, rather than the method described in Fig. 6, principally on account of the enormous saving in labour thereby effected. When the ground was sufficiently solid, the stability of poles fixed in bored holes was sufficient to resist all ordinary pressures. He stated he had that day inspected the length of line described at the end of the paper as having been erected in bored holes, and he found that though the line carried heavy No. 4 wires not a single pole had moved, and not a shilling had been spent on it since its erection, although the gales of three winters had blown over it. He thought that was a fair criterion of the stability of lines erected in the manner described.

Mr. Graves: Some years ago I was under the impression that it was quite practicable to supersede the use of the spade and pickaxe by the introduction of mechanical means for lessening the labour of making the holes, and at the same time to reduce the quantity of earth disturbed. So far as experimental trials went, I should be justified in saying something in favour of the borer, and more so of the Spanish spoon. I made a trial of this instrument near Coventry. The day was spent in operating upon different kinds of soil, sometimes with men using the shovel, and sometimes with the borer. Except in rock and hard gravel the borer showed an advantage. Its performances were clean, and the work was done quickly; and it was found possible, with care, and people watching, to lower the poles into the holes without difficulty. On the other hand, in that trial certain drawbacks developed themselves and proved serious. One was the greater amount of physical labour attendant on the use of the borer. So soon as a certain depth was reached—say over 2 feet—every lift became to the men a source of continual hard work. There is no relaxation from it. It is a mill-horse round, followed by a heavy lift, and it was clear from the first day of trials that the instrument was, on that ground alone, likely to be unpopular with the men. My experience has been, that, when you

try test experiments under the superintendence of officers interested, you get the best results possible; the circumstances are the most favourable under which experiments can be made. I have used the apparatus of both kinds exhibited here—the borer and the spoon —and I have been asked for reports as to their success. During the first month or two of a few various trials, I was enabled to report favourably of both instruments; more in favour of the spoon than of the borer; but six months afterwards I found that, except in a few instances, the whole of these instruments dropped out of use. The borer, in spite of advantages which are manifest, has serious disadvantages, which do not seem to have been sufficiently met by recent modifications. It is said that the surface disturbance of the soil is less by the use of an instrument of this kind than in ordinary operations, and that a smaller portion of the natural soil is interfered with than in the course of ordinary operations. This I concede. These advantages, however, cannot always be availed of, and real evils arise from the labour required, and from the difficulty of working the instrument except in open spaces. A large proportion of the work of the Post Office Telegraphs is close to the bottoms of hedges, or on the borders of canals. In those cases it is impossible to work by men traversing round a circle, and in such cases the use of the ratchet would be involved, resulting in a great reduction of the speed of the operation, very much indeed. The Spanish spoon, so far from being abandoned, is in my division the only one of these instruments virtually in use. We have made some modifications in its shape, but they are not very important. The general result is you can work the spoons in a limited space where you cannot obtain room to use the ordinary spade and pickaxe. In the case of a corner or angle of a wall space is valuable, and there the other instrument cannot be employed. Moreover, in comparing the performances of the spoon and borer in loam or clay, where the borer cuts easily, the difficulty is to lift the earth out. In the case of hard gravel or soft rock, for practical purposes the borer becomes a spoon. You smash the material with the bar and then lift it out, and that is more easily effected by the use of the spoon. I may say that after several

experimental trials, in which I convinced myself that the spoon was more generally available, I determined to try whether we could not have a long length of line erected by means of that agency. From Birmingham to Derby, some forty miles, I arranged for the work being done with the spoon only. Amongst thirty men, there were only about three spades altogether, and I may say the whole of that line was erected by the use of the spoon. Two foremen of experience, with sufficient interest in the work to compel the use of the spoon, were in charge. The result was, that the holes were got out in a satisfactory manner, and without more labour than is experienced with the pickaxe and spade; but beyond that I consider the economical gain was *nil*. The work costs no more, but it costs no less, than under ordinary conditions. You have not to remove the same amount of soil, but what you gain by the saving of labour in the removal of the earth you lose from the increased labour in fixing the pole. It is assumed that the pole will be lifted accurately, and that the men will make no mistakes, but in many cases the sides of the hole are broken in, and the pieces have to be spooned out again. In the case of very heavy poles, it is necessary to collect a greater number of men to lift the timber than when it is slid into the ordinary hole made by the pickaxe and shovel; therefore I consider, looking at the pounds shillings and pence part of the question, it costs no less than erecting the line in the ordinary manner. I would add, that although these results were obtained by men working in large gangs, under comparatively effective supervision, I do not find it practicable to enforce the use of the borer, or any other substitute for the spade, in the case of a few men working by themselves. The instrument the workmen cling to is the spade. The South American experience of Mr. Treuenfeld is the same as our own in England, viz.: that where you have the power to compel the use of new machinery you may succeed, but where the men are left to their own control, and are few in number, you fail to get the advantages which are derived from working in larger bodies. So far, therefore, from being a success, I am sorry to say that neither the borer nor the spoon are used in the country I am acquainted with to any great extent; but that

there is between them this difference—the spoon lingers, and may revive; the borer is considered practically defunct.

Mr. R. von FISCHER TREUENFELD said: Having had some experience in earth-borers, I beg to state a few disadvantages with regard to this mode of operation which I do not see mentioned in the paper. The paper says, one of the great advantages of the boring is the greater solidity of the poles when erected. I suppose this is assuming the earth is properly rammed in, but I have found difficulty in doing this. Supposing the bore is 10 inches diameter, and the pole 7 or 8 inches diameter, there will be very little space between the pole and the ground, and very often the pole will not stand in the centre, or perhaps the pole may be crooked, and there may be places where it is difficult to use the rammer, and in clay soils it is very difficult to introduce the earth properly between the narrow space in the ground. If this is not properly looked after by the men who have charge of the work, it will be found that open spaces will remain between the poles and the natural ground, and these open spaces will destroy the poles, and, if those spaces get filled with water, it will tend to destroy the poles, which is not the case if the holes are dug in the ordinary way. I also think it is a disadvantage to have to use so many tools, especially where long lines have to be erected, as in Australia or Russia, in which cases it is important to reduce the number of tools as much as possible. In Paraguay, on introducing these borers, which I took with me in 1863, I found that the men who were good workmen would rather do the work without the borers than with them, and I had to give them up very soon, against my will, and the men did the work better without them. I quite agree with the advantages mentioned in the paper, but I thought it well to mention the disadvantages I have noticed.

The PRESIDENT: May I ask what tool it was you now preferred to do without?

Mr. TREUENFELD: I had the same description of borer as that exhibited here, but not so long. The men preferred to dig the hole with the pickaxe and shovel. The lines in Paraguay were chiefly military lines. The men were well trained and very clever

workmen: still they did not like to work with the borer, partly, no doubt, for the reason that they had to take so many tools. The paper says it is in all cases necessary to carry the pickaxe and shovel.

There are certain places where the borer cannot be used, consequently the men have to carry a great many more tools, which is very disagreeable. If you have to go over a great many miles a-day, and have to shift the tools from one place to another, it is important to reduce the tools to the smallest quantity and the simplest character possible. This may have been the chief reason why the men did not like these borers, but, on the whole, I think they did the work as quick with the shovel and pickaxe as they would have done with the borer.

I omitted to mention another disadvantage I experienced. I never used sheer-legs for lifting wooden poles, because it added to the number of tools to be carried, so that the poles were lifted by hand. In putting the pole into the hole it often happens that it knocks against the sides of the hole, and pieces of earth tumble down before it reaches the bottom. The men, not seeing this, imagine the pole to be down the full depth of the hole, whilst it may be six or more inches less, from the soil that has fallen in. They begin ramming down, and afterwards find that the pole has not been put down the proper depth. This I have repeatedly seen. I may mention that the borer answered very well when used for poles of smaller dimensions, for erecting Military Field Telegraphs. With small poles of about 5 inches diameter, being employed expressly for a Military Field Telegraph line, the borer answered very well indeed, and there was no difficulty in lifting the poles and dropping them clean into the holes. For such purposes as that I think the borer may be fairly recommended.

Mr. Bell: Mr. Gavey has, I think, somewhat overstated the quantity of earth that has to be removed from these holes; he gives the width as 2 feet at the top, whereas I think 1′ 6″ is ample for a man to work in, and that reduces the quantity of earth to be removed from 30 to about 21 cubic feet. There is one point on which we require further information, and that is with regard to

the stability of A poles without a sill or a tie-piece near the base. It is assumed that the friction of the earth on the poles is such that if they were canted over by a heavy strain the earth would move in the direction shown by the curves in the diagram; but unless the pole-butt is bell-shaped, or tapered, I don't think the earth would have sufficient hold to prevent canting. The height of the poles being so much greater than the width across both at the base, the one would go down and the other would be lifted; that is where there is no sill-piece. When the strain comes on, C would be lifted up till the pressure comes on the other side at the top, and in the opposite direction at the bottom. If there is thus a movement of one inch on one side or one inch on the other, it would add considerably to the extent to which the top might yield by the vertical movement when subjected to a severe strain. Mr. Langdon appears to have formed the same opinion as I have done of this plan for A poles, for in his letter to Mr. Gavey which has been read he recommends that iron rings should be fixed on the poles near their base. It is therefore necessary to adopt something of this sort to give the poles a good hold of the soil. Without the sill-piece, or unless the timber is bell-shaped, it would support very little of the weight of the soil.

Mr. Charles Burton (Director of Telegraphs in the Argentine Republic): What has been said with regard to the earth falling down into the holes when the poles are put in, is a matter of fact in my own experience, even where the holes are larger than would be made by any of these instruments before us. It is not only not an advantage to have the pole higher out of the ground than it should be, but it is a disadvantage to have to take the pole out to remove the earth which had fallen down the hole. In the country in which my operations have been principally carried on, there are not the same facilities for obtaining poles all of the same form and dimensions, that there are in this and other countries. Although iron poles have been used on the greater portion of the lines under my charge, I have something like 30,000 wooden poles, and out of a hundred poles you will not find half a dozen straight ones, so that the borer would be practically useless, and, if the holes were bored

of sufficiently large dimensions, the ramming down would be a matter of serious difficulty, and would leave cavities which would be filled with water, and materially injure the poles; or, if no injury was done to the wood on account of its naturally hard condition, yet it would cause the poles to loosen, and you would have to be continually employed to keep the poles straight, especially on a curved line.

Mr. W. H. PREECE inquired what was the nature of the wood employed on the Argentine lines?

Mr. BURTON: Very hard indeed, almost as hard as iron. There are two kinds generally used, viz., the *algarroba* and the *quebracho colorado*. Sometimes *quebracho blanco* is used in mistake for *quebracho colorado*, it being difficult for a person not well acquainted with the colour of the wood to know one from the other; but the *quebracho blanco*, unlike the *colorado*, deteriorates by contact with water, consequently the cavities which might be left in the ground in refilling the hole would more materially affect the *blanco* than the *colorado*.

Mr. W. H. PREECE: What do you consider to be the life of a pole in that country?

Mr. BURTON: By employing the best kind of wood, judging from what I have seen used in farms and buildings, the poles cut from the heart of the tree would last for 50 years probably, either in a perpendicular or horizontal position, without suffering any deterioration whatever. The wood itself is so heavy, that, owing to the great cost of transport, it is cheaper in the capital of the Republic to use wood obtained from Europe or the United States, the roads of the country being as yet in the most primitive condition, and no railways available for the transport of the wood from the forests where it is grown.

Mr. W. H. PREECE: Is the timber very abundant?

Mr. BURTON: Yes, in the northern provinces you find forests of 100 miles in extent, and even more, and then perhaps a little broken ground with pasture land, and then more forest. I dare say for an extent of 700 miles you would find this wood in abundance.

Mr. W. H. PREECE: So that practically it is very cheap?

Mr. BURTON: It is; at the commencement of my operations the poles could be bought for 10*s*. each, including carriage and distribution about the line laid out, but afterwards the price increased; not from the use of the wood for general purposes, but because the proprietors of estates knew the telegraph poles must be had, and they raised the price accordingly. It being impossible to buy the whole of the wood at once, the price was increased to about 30*s*. a pole, including carriage and distribution.

Mr. W. H. PREECE: Have you any idea at what age the wood is used?

Mr. BURTON: I am not able to say. The tree grows in some places along the Paraná coast as well as in the interior; but it is useless to endeavour to bring it to Buenos Ayres, because land-carriage would be too expensive, and from its great specific gravity it will not float in water, so that it cannot be brought down in rafts. A patent was taken out for a cylinder pontoon, to be employed as a raft, but it has never been brought into practical use, so that they are without this wood in Buenos Ayres, except in small quantities.

The PRESIDENT: I understand from you that no form of borer has been found suitable for the Argentine Republic, and that the holes are made with the pick and spade.

Mr. BURTON: I believe this class of borer was tried by one contractor, but he found it useless in consequence of the very irregular form of the poles, and also in consequence, as Mr. Treuenfeld says, of the dislike of the men to carry so many tools. I have myself had to dispense with tools which would be used in England were the means of transport easier. Even for repairing operations, we require that the tools should be as few and as simple as possible, and also small and easy of conveyance from place to place, because the men have to travel on horseback sometimes a considerable distance in a day.

Major MALCOLM, R.E.: It has been suggested that this borer would be useful for military telegraphs. I would ask Mr. Treuenfeld whether he has used it himself for that purpose, because some time ago I tried these instruments with a view of introducing

them for military work, more particularly the small borer spoken of, but the manufacturer said there was difficulty in making them small, one reason being, that with the V-shaped opening at the end very small stones would jamb, and the tool would have to be repeatedly pulled up to clear it. Another thing which in my small experience was against the use of the borer—I don't know whether others have found the same—was, the tool broke in the pipe.

Mr. TREUENFELD: The borers I used in Paraguay were 5 or 6 inches diameter, and were shorter than those now exhibited—say 4 feet long, but stronger than those shown to-night, and there was a ring on the top for the handle.

Mr. W. H. PREECE: What kind of soil was it?

Mr. TREUENFELD: Loam and sand. It was easy to bore 3 feet deep in the clay, but going 4 or 5 feet deep there may be difficulty in raising the pole; but at the depth of 3 feet I did not experience any difficulty. I have used the borer for military telegraphs to a depth of 3 feet, with poles of an average diameter of 5 inches.

Mr. CULLEY: I have given much attention to boring and digging tools; and about three years ago I distributed Marshall's borers, and the bar and spoon, widely throughout the Postal system.

Reports as to their value compared with the pick and shovel have been received, and they are very contradictory, as was naturally to be expected when a new process was in question.

There are Conservatives and Liberals in the arts as well as in politics; there are careful experimenters who expect failures at the commencement of every new process; and experimenters who condemn a process if it is not successful on the first trial. And there is also the objection of working men to anything new; and the fact is that great tact and perseverance on the part of the engineer and his foreman, coupled with the belief that the new method can be made to succeed, are necessary, in order to overcome this objection.

Several of my reports state that nothing could be done with either spoon or borer; others show that very good work was done with both.

A report from Nottingham states:—

"In loose sandstone, which, although like sand, is much heavier

and more tenacious, 2 men bored 1 hole 5ft. 6in. deep in 13 minutes, with 6 lifts, and set a 24ft. larch pole and punned it down in 6 minutes more.

"In gravel and water 2 men bored 1 hole 5ft. 6in. deep in 40 minutes with 11 lifts; but much loose stuff kept falling in the hole; they set a 24ft. larch pole and punned it down in 6 minutes more: the water was not baled out, and the pole when punned, though not firm at first, was found next day to be quite firmly set.

"In clay, 2 men bored 1 hole 5ft. 6in. deep in 30 minutes, with 14 lifts, the most difficult work being the lifting, which, although the valve was used, was very hard work."

It seems certain from this report that, in the absence of apparatus for lifting the borer, its use involves very hard and disagreeable work. It was lifted 14 times in boring a single hole, or about 3 times in each foot. The weight of soil lifted was perhaps trifling, but it formed as it were a piston working with immense friction in a cylinder which, towards the end of the operation, was 5ft. in length. Besides this friction, there is the resistance of the air passing through the valve and the soil into the space below the piston.

The same objection does not apply to the bar and spoon. The work is not hard, although it may be tedious.

A report from Mr. Evans, of Birmingham, states:—

"The tools were tried at Coventry, and on the common we had no difficulty whatever in getting out holes of a size sufficient to admit a 35ft. and 40ft. pole without having the hole above 2 inches wider at the top than was sufficient to admit the butt of the pole.

"It was at first anticipated that we should have some trouble in rearing poles of the lengths mentioned, but it was found, if one side of the hole was made a little wider for about 18 inches in depth, so that the butt-end of the pole could more easily slip into the hole, that by placing a crowbar at the opposite end of the hole to that at which the butt-end of the pole was inserted, half a dozen men with two ladders could rear the pole and hold it sufficiently firm to allow it to simply drop into the hole, where, without any assistance, it stood erect. When this operation had

been completed two men were sufficient to pun and fix the pole firmly in the ground, *i.e.*, one man to steady and balance the pole if necessary, the second man to throw in the earth and pun it down.

"The new line, Birmingham to Derby, was under my own immediate superintendence, and from Birmingham to Fazeley, a distance of 14 miles on the canal, and from Fazeley to Burton, a distance of 18 miles on the road, I used nothing but spoons and bars for the work, and always found them to answer admirably, giving less work in labour to get out the hole than the pick and spade, and, as a matter of course, less labour than under the pick and spade in filling in the holes; added to which is the advantage of having the poles more firmly set in the ground, than could possibly be the case where a large hole is dug, and the pole made firm by filling up with loose soil and punned in the ordinary way.

"A further additional recommendation to the spoon and bar is, that the surface of the earth in which the pole is to be placed is only disturbed to an extent of little more than two or three inches diameter at the ground line, thus making cleaner work, and leaving the earth in almost as smooth condition as before it was disturbed.

"In carrying out the work above mentioned, I came across ordinary soil, gravel, solid rock, and clay, and no difficulty whatever was found in getting out the holes. It may be mentioned we used tubular iron bars pointed with steel for ordinary earth, sand, and gravel; but for rock we used a solid iron bar chiseled $\frac{3}{4}$ tip.

"Between Stafford and Newcastle, when renewing that line in 1871, spoons and bars were also used, and in one particular instance, south of Stone, we came across a considerable length of rock, the holes in which we got out quite as soon as, in fact quicker than, if we had used a pick and spade.

"I wish to add that in the first experiment with the bar and spoons I was ably assisted by the foreman, T. Davies, who exercised great care in carrying out the instructions given him, and who, throughout the whole of the work under his care, since the introduction of the bars and spoons, has never used any other articles for planting poles.

"I found, however, that with other gangs and individual workmen there was the greatest dislike to the use of the bars and spoons, and the men would not, unless under compulsion and personal supervision, use them.

"My own opinion, from considerable practice of these articles, is that they are invaluable both as regards economy and labour, strength of the pole when planted, and neatness of work.

"I also, under my own superintendence when we were renewing the west road line between Birmingham and Tewkesbury, saw these tools perform an amount of work that could not be done with the pick and spade.

"The bars used for boring or breaking rock are made of solid iron, 6 feet 6 inches long; both ends are tipped with steel for about 3 inches, and one end is pointed so as to form a diamond drill; the other end is shaped like a chisel and is used for shaping the hole at the bottom; the drill end is for breaking or boring the rock. The bar used for ordinary ground is 8 feet in length and tubular to within about 6 inches of the top and bottom; one end is tipped with steel for about 3 inches and made in the form of a chisel 2½ inches wide; the other end is solid iron and is used as a punner.

"The shapes of the spoons first received were not good, and I found they could be rendered more useful with certain alterations, according to the skill, and in some instances fancy, of the man using them."

This report of the spoon and bar may be very fairly paired with Mr. Gavey's paper. Each of the experimenters intended to make his special tool succeed, and in consequence its use was urged perseveringly until the *vis inertiæ* of the workmen's conservatism was overcome.

The borer was used in America as early as 1859. Shaffner mentions it in his book on telegraphy published in that year. I have been informed by one of the principal officers of the Western Union Telegraph Company of the United States that he never used the pick and shovel, but employed the bar and spoon solely. The spoon was first brought to my notice by Mr. E. G. Bartholomew, who used it in erecting a line in Spain, and afterwards one

across the moors from Tavistock to Dartmoor Prison some ten or twelve years since.

The great difficulty after all in the use of these tools is that the men have to be *trained* to their use. Any labouring man in the country can use a pick and shovel with more or less skill. You tell him to dig a hole and he knows how to do it; but if you introduce the borer or the spoon you must first get over the objection of the men to anything new, and then you must teach them the use of the tools, and then you must give them sufficient time to practise with them, so that by the time they know their proper use the work is finished. The trial of these tools will not be given up, and, although I may have less hope of their being ultimately successful than I had three years ago, yet, when I recall the difficulty we experienced in substituting the use of ropes and blocks for the system of pulling wires up by main force, and when I recollect that the same objections as to extra trouble in carrying tools was then urged, I do not despair that energy and perseverance will introduce some form of digging-tool better than the pick and shovel.

Mr. TREUENFELD: Mr. Culley says he does not see any difficulty in lowering the pole clean into the hole and leaving a space of 2 or 2½ inches round it, but I say the difficulty is that the pole may not stand exactly in the centre: there may be 3 inches on one side and only one inch on the other, and then there will be difficulty in ramming the earth in properly; and, even with 2 inches clear round the periphery of the pole, I fear there would be some difficulty in ramming the earth round the pole.

Mr. GOLDSTONE: Being interested in the fixing of poles, I would say one or two words, more as a warning to gentlemen paying attention to the designing of earth-borers than anything else. I was so taken with some experiments which I made, and so satisfied with the results on level ground, that I placed a set of Marshall's borers in the hands of a foreman, who was engaged in constructing a line of poles by the side of a railway, and gave him some skilled labourers to work the borers. It answered well on level ground, but it was another thing when employed on the side of a bank of any description, when the borers had to be abandoned.

It is therefore evidently necessary for those who design implements of this description, in order to be successful, that they shall make something which is capable of universal application, and which can be used on any form of ground. Railway Companies are much interested in this matter, and, if a borer is devised which can be used on the sides of railways, it will be a very useful thing. In many cases banks are made of new tipping, and the soil is light, so that you have to make a great hole, as well as a platform to put the earth on which is taken out of the hole. If the borer could be adapted to those circumstances it would save a great deal of labour.

Mr. Gavey (in reply) said: Amongst the objections raised against the use of the borer, one of the first is that it involves the carriage of a large number of additional tools. I quite admit that if a gang of men have to add the weight of boring machinery to that of their ordinary spades, pickaxes, &c., the labour becomes so great, that it may lead to the desire to abandon the use of the borer; but this is by no means necessary, for it is to be borne in mind, that, in a country like England, the officer in charge of the line generally knows the character of the soil through which he is going to erect his telegraph, either from personal observation or through the surveys necessarily made, and he acts on that knowledge. If the country admits of boring operations, boring tools are issued; if not, then provision is made for the work being done by the ordinary means of pickaxe and shovel. Another point raised is, that the small space left between the pole and the ground does not leave room for the proper ramming in of the earth. I must confess that I have myself regarded this small space as one of the greatest advantages of the system, because, if there is a large mass of loose earth to be filled in, it is almost impossible to consolidate it properly, whereas, if you have the soil inclosed in a narrow space between the solid sides of the hole and the pole, you have an opportunity of consolidating it and making it as firm as it was before its disturbance. It is further said that a bored hole does not admit of a crooked pole being properly set in it. That difficulty, when it arises, is obviated by the use of the bar exhibited here. The sides of the hole

are cut down to suit the pole, and both in this case and in that in which the pole quite fills the hole sufficient room is obtained to admit of the earth being firmly rammed, without that danger of injury to the men's hands that has been adverted to. Again, the use of the borer abroad has been referred to disparagingly. My own remarks referred more to the use of the instrument at home, where the officer in charge can always exercise his discretion as to what tools he will use in any given work. Abroad it is very different. Officers in charge of such lines do not enjoy the facilities of carriage, nor have they at their command the appliances, we have in a country like England. In such cases it may be advisable to use tools which will be applicable and useful under all circumstances. Such a fact does not however derogate from the utility of the system in a highly civilised country.

With reference to the remarks of Mr. Graves as to the use of the Spanish Spoon proper, and the borers described in the paper, I am not aware whether that gentleman has used the small modified borer which I referred to as Marshall's Spoon. That is an instrument well adapted to overcome the objection raised to the borer itself. True, with a heavy borer you cannot open a hole near to a wall or in other confined spaces; but with a small ratchet-borer the difficulty is overcome. It will bore freely in clay and light soil; in heavy soils it can only be used as a spoon, and, in my opinion, with this instrument you can collect the soil far more readily than you can with the Spanish Spoon.

Another point is with reference to the A pole. I understand from Mr. Bell he argues that the force which retains the A pole, illustrated by fig. 6, in its vertical position, when exposed to a lateral strain at the apex, is simply due to the friction between the soil and the sides of the pole. That would be nearly so if the pole were raised vertically out of the ground by means of sheer-legs, but I think that, when moved horizontally, his view is scarcely the correct one. If you assume the triangle to be perfectly rigid you can treat it simply as a bent lever, the fulcrum being placed at D or B, fig. 6, or, more properly speaking, at a point intermediate between the two. If we take it at D, then, to move the pole over

by lateral pressure at A, you have to raise the whole of the earth in the direction shown by the dotted lines. If, on the contrary, you take B as the fulcrum, you have to raise the earth in the direction shown by the whole lines, so that in neither case would the pole be held by mere friction alone. As the poles are never thoroughly rigid, no doubt friction between them and the soil plays an important part in retaining the structure in its position, but not by any means the sole or even the principal one.

With reference to the general use of the borer, no doubt two of the greatest objections to its general introduction are, first, the physical strain involved in withdrawing it from the soil; and secondly, the difficulty in the case of heavy poles in raising them into the holes. These difficulties can be fairly met by the means I have described in the paper, and other contingent advantages are at the same time obtained. The addition of light sheers admits of the use of the borer and the raising of the poles with less labour to the men than in any other method of erecting telegraph lines, and the mere transport of the sheers from pole to pole, in addition to the ordinary boring tools, is simply effected by means of a light truck. In this manner the only objection that remains in the mind of the men is that of novelty, and when this alone is to be overcome it is simply a matter of discipline.

Mr. Culley: Then every time you bore a hole the sheers would have to be moved to it, and they would be taken down ready for the next hole.

Mr. Gavey: Yes; the sheers would be erected over each hole and would remain *in situ* till the pole was complete. With light sheers, 21 feet long, that would be a work of little labour, and two pair of sheers would suffice for a gang of six or seven men. The time lost in shifting the sheers is very trifling indeed, not exceeding two minutes for each pole. The poles are dropped in vertically by the same means, and the falling of *débris* into the hole is entirely avoided.

Mr. Graves: How many men would be engaged in that operation?

Mr. Gavey: Seven men were employed in the case illustrated.

Mr. Graves: Seven men at the same hole?

Mr. Gavey: No; three men at one hole and four at another. These two gangs each had a complete apparatus, which was shifted from hole to hole as they went on. Some excess in the weight of one pair of sheers made the additional man necessary in one gang.

Mr. Graves: Have you calculated the cost of that as compared with the ordinary way?

Mr. Gavey: I have not myself had sufficient experience on a long line with the borer to give a decisive opinion on that point. I have not erected more than three or four miles of continuous line in bored holes, the results of which are given in the paper, and these, so far as they go, are in favour of boring in pecuniary as well as in other respects.

Mr. Bell: Had you any trouble in erecting 24 or 30 feet poles in gravelly soil from the gravel falling into the holes? or had you any sheath for guiding the pole?

Mr. Gavey: I have found no difficulty in doing so. In rearing poles by the ordinary means—that is, without sheers—it was the practice to put the punner-bar at the extremity of the hole to act as a fulcrum. No sheath was used, but, as almost all the poles were reared with the aid of sheers, the difficulty, if it existed, was entirely overcome.

Mr. Bell: In gravelly or other light soil, without the use of a sheath it is almost impossible to imagine that the pole can be placed in the hole without a considerable quantity of soil falling into the hole.

Mr. Gavey: My experience has not been in that direction. A small quantity of soil might be knocked in, but that would be met by boring the hole six or eight inches deeper to allow for that. The soil, except in that of the lightest description, would be firmer around a small bored hole than at the edge of a large dug one.

The President: I wish to make just one remark upon Diagram No. 6. The disadvantage of the method shown in No. 5, as I understand it, is the large quantity of loosened soil that it involves. The advantage it presents is the presence of the cross-bar, which cannot move upwards without dragging a large quantity of soil with it. But, on referring to Diagram No. 6, I think that, not-

withstanding the comparatively small quantity of soil loosened in the process of executing it, we have an arrangement which could not possibly be advantageous, even with the addition of ratchets projecting out, as in the additional (not numbered) diagram before us. It appears to me that in Diagram No. 6 we have a mechanical arrangement devised, as it were, for the purpose of plucking out one or other of the poles. As the arrangement is described in the paper, it is stated that, if the fulcrum were B, the lines of motion would be those shown by the full curves; or, if the fulcrum were D, the lines of motion would be those indicated by the dotted curves. Probably, in the actual case, the virtual fulcrum would be somewhere between the two, B and D. The line of motion, then, of the middle of the immersed part of the pole C would be vertically upwards. There would be a slight obliquity to the right in the motion of the upper end of the part immersed, and a slight obliquity to the left in the motion of the lower part; but those obliquities would be so slight that it would take a very solid surrounding to give any considerable resistance to the motion. It seems to me that, after being tried for a time by severe forces pulling in the direction of the arrow-head in the diagram, the left-hand pole would become loose, and would then have only its mere weight with which to give stability to the structure; and, as the same thing might happen to the other pole by forces in the other direction, the whole is, in point of fact, an arrangement as if devised in the first place to loosen the hold of each pole upon the earth, and afterwards pluck out one or other, according to the direction of the final pull. It is easy to reckon how much stability will be given by weight, and I believe the virtual stability of this arrangement will be that of two loose poles standing simply by their own weight. I suspect that, in truth, there would be less resistance in the case of the two poles rigidly connected (as shown in the diagram) than there would be if they were quite independent, with merely a loose link at the top to give a resistance equal to the sum of the resistances of the two poles separately. I think it very probable, that, even taking into account the assistance which gravity would give, it would be found that the particular arrangement shown in the diagram would

have more power of resistance to a force applied at the top in a direction perpendicular to the plane of the diagram than to a force in the direction shown by the arrow-head.

In reply to Mr. Culley, the PRESIDENT further said: If the force was always in one direction, then there would be nothing to loosen the pole B A. The pole B A would give its full resistance. I think that, if there were forces sometimes in the one direction and sometimes in the other, it is even possible there would be less resistance with the two poles than from one alone, from the fact that the pole A C would get loosened by forces acting in the direction of the arrow-head. It seems to me that after both poles become thus loosened the resistance might be less than with a single pole. If the force is always in one direction (as that shown by the arrow-head), then, with the poles loosely joined at the top, the resistance would be greater undoubtedly than with a single pole, but probably not quite so much greater as double. Although that is not a good arrangement, it would be better than the arrangement shown in the diagram. I think that if the poles were close side by side there would be less than double the resistance of one pole, because the whole amount of the resistance of the earth that one pole would experience would be diminished by the presence of the other. If the two were placed at a considerable distance from one another, and if they were not coupled so as to give the leverage action upon which I have commented, then the force would still be not quite equal to the sum of the two, because the whole force would not be distributed exactly on the two poles in proportion to their maximum resistances. The leverage action in the ordinary hammer-fork for drawing nails is almost precisely the same in principle as that which I have indicated in this case.

I have listened with very great interest both to Mr. Gavey's paper and to the comments, and also to the general information with which other speakers have followed it up. Although it is quite clear that in distant countries, and in a variety of soils, and in circumstances in which division of labour and minute subdivision of appliances are not available, the method of making holes by the pick and spade is that upon which most reliance must be

placed; still I think that Mr. Gavey has made out an exceedingly good case for the borer in a very large class of applications. What Mr. Culley has said with reference to the trials that have been made shows that there is a large province still left for the borer. It must be remembered that the method is comparatively new, having been in use only a few years; and when so much can be said in its favour as Mr. Culley has said, and has been brought forward in the paper, I think we must agree that it is a very important appliance. In a large district of country over which a great many lines of telegraph may have to be taken, and where the soil is generally of a suitable character, the borer will be of considerable value. There can be no doubt whatever of the figures which Mr. Gavey has put before us, and they show a very great increase in the quantity of work done by the labours of a certain number of men through the use of the borer in favourable circumstances. The advantage of bringing forward and describing new applications before this Society is well illustrated by the information and new ideas which we have received from Mr. Gavey himself, and the light which has been thrown on the subject by the different speakers. I propose a cordial vote of thanks to Mr. Gavey for his paper.

The following Candidates were balloted for and declared duly elected :—

Foreign Members :—

J. M. Collette	The Hague.
E. H. Johnson	Philadelphia, U.S.
G. A. Moore	Erie, Pennsylvania, U.S.
W. J. Phillips	Philadelphia, U.S.
C. T. Sellers	Reading, Penn. U.S.
W. H. Spang	Reading, Penn. U.S.
Don Carlos Soldan	Lima, Peru.
Marquis L. Vianesi	Messina.

As Members :—

W. R. Brooke . . .	Bombay.
W. J. Conningham . .	Adelaide.
F. Despouites . . .	Submarine Telegraph Co.
Edward Gilbert . . .	Tokei, Japan.
J. M. Lane	Calcutta.
W. F. Melhuish . . .	Calcutta.
Sidney Montefiore . .	Melbourne.
F. D. Nelson . . .	Teheran, Persia.
W. Williams . . .	Calcutta.

As Associates :—

J. Jeffery	Marseilles.
T. Y. Johnstone . . .	Ispahan, Persia.
F. S. Joseph . . .	Brixton.
J. K. Logan . . .	Dunedin, New Zealand.
R. J. Lewis	General Post Office.
J. Oppenheimer . . .	Manchester.
A. Schindler . . .	Teheran, Persia.
Louis Sterne . . .	Westminster.
C. J. Stokes . . .	Brompton.
L. E. Thornton . . .	Calcutta.

The Meeting then adjourned.

The Thirtieth Ordinary and Third Annual General Meeting was held on Wednesday, the 9th December, 1874, Mr. LATIMER CLARK, Vice-President, in the Chair.

The CHAIRMAN announced that the Ballot for President and Members of Council and Officers of the Society for the ensuing year would take place, and be closed at half-past 8 o'clock.

Messrs. R. von Fischer Treuenfeld and William Langdon were appointed Scrutineers.

The Secretary read the Annual Report from the Council as follows:

ANNUAL REPORT FOR THE YEAR 1874.

GENTLEMEN,

For the third time your President and Council come before you to give up those offices, which have been during the past year held at your request; and in resigning them to place before you some account of the operations of the Society during the past year.

We have continued thoughout the year to enjoy the valuable advantage of holding our Meetings in the Institute of Civil Engineers; and the success of the Society must in a great measure be attributed to a privilege so valuable, which has been accorded to us so kindly and so freely in the past by the President and Council of the Institute of Civil Engineers, and which we trust will be continued in the future as it is at the present, until such time as the Society has established itself on such a basis as will enable it to possess a large room of its own, wherein the ordinary Meetings of the Society may be held. The rapid progress the Society of Telegraph Engineers has made has greatly pleased the parent institution, and the future progress of the Society will be watched by them with interest.

We cannot do better than repeat the following remarks on the

subject made by our first President, Dr. C. W. Siemens: "I consider it a most fortunate circumstance for our Society that, through the liberality of the Council of the Institution of Civil Engineers, we are enabled to hold our first Meetings in these commodious rooms, under the roof of the parent Institution of Engineers, and with their good wishes to cheer us on our way. May our success justify their liberality."

The Council have, under the direction of the Committee, furnished the rooms taken by them at 4, Broad Sanctuary, as a Library and an Office. The Library will, it is to be hoped, be soon supplied with a valuable collection of books, and will also be used as a Reading-room, where all the scientific journals and periodicals may be seen, including the principal foreign publications. Arrangements are now being carried out with that object, and it is anticipated that, when that result has been obtained, Members will find the Reading-room and Library a most valuable assistance in their professional pursuits.

Negotiations have been going on during the past year relative to the acquisition of the Library collected by the late Sir Francis Ronalds, whose loss it was our painful duty to record last year. This library is considered without exception the most perfect technical (as regards Electricity and Telegraphy) library in the whole world; and certain Members of your Council have been unceasing in their efforts to bring this to a satisfactory conclusion, and they hope shortly to announce the actual acquisition of the library. The Society is greatly indebted to Mr. Samuel Carter, who now possesses the library, for the interest he has shown in the Society, and for the invaluable gift of the library left to him by the late Sir Francis.

The continued progress of the Society forms a subject of congratulation, for the increase of Members is of a most satisfactory character. The fact that at the last two Meetings 71 candidates were proposed is a sufficient indication of the prosperous condition of the Society, and of the high position it has now assumed. Altogether, the increase during the year may be considered most satisfactory. This, calculating three gentlemen who will be elected to-night, amounts to 126, being Members of all classes.

The following statement gives particulars of the number of Members at the end of the several years:

	1872.	1873.	1874.
Honorary Members - -	0	3	4
Foreign ,, - -	25	57	81
Members - - - -	155	185	202
Associates - - - -	170	270	346
Students - - - -	2	7	15
Totals - - -	352	522	648
Increase - - -	——	170	126

It is satisfactory to observe that the increase in the various classes continues most favourable, and it is gratifying to notice that the accession of Foreign Members is also on the increase. Your Council would wish to put out that amongst the latest additions to the class of "Foreign Members" a large number of the Telegraph staff of the Argentine Republic has been added to the Society by the recommendation of Mr. Charles Burton, Director-General of Telegraphs and the Society's Honorary Secretary for the Argentine Republic. Amongst the accession of other Members may be mentioned the praiseworthy example of the Brazilian and the Eastern Submarine Telegraph Companies, who have nominated their various Superintendents and Electricians as candidates for the Society, giving the world a most favourable proof of the position occupied by the Society, and of the benefit and good it is calculated to confer upon the Officers of large Telegraph Companies. It is a matter of regret that some few names will cease to be seen on the list of our Members, and we cannot leave the subject without a notice of the sad loss the Society has received in the death of a Member who was well known. We allude to Mr. Rickett, one of our early Members, who unfortunately met with an untimely end in the execution of his duty by the foundering of the steam-ship "La Plata" in a recent heavy gale.

The Council have been at some expense in fitting up and furnishing the rooms at 4, Broad Sanctuary, as Library and Office, and on the presentation of the accounts it is probable the cash balance will not be so favourable as last year; it must, however, be remembered that the amount of money so expended will stand as assets in the Society's books.

Difficulty is experienced in the collection of subscriptions, and the Council must urge upon Members the importance of their subscriptions being forwarded in due course.

During the year the Council have made the following additions to the number of Local Honorary Secretaries:—

Don RAMON PIAS, Director-General of the Chilian Telegraphs, Santiago	CHILI.
CHARLES BURTON, Director-General of the Argentine Telegraphs, Buenos Ayres	ARGENTINE REPUBLIC.
C. L. MADSEN, Great Northern Telegraph Company, Copenhagen	DENMARK.
J. M. COLLETTE, Engineer of the Netherlands Telegraphs, The Hague	NETHERLANDS.

Your Council are about filling up the vacancies for France and other places, and it is hoped that shortly the most important Telegraph countries of the world will be represented by the Society's local Secretaries. It may, however, be satisfactory to observe that the appointment of Local Honorary Secretaries has been productive of much good to the Society.

The following is a list of the gentlemen who now occupy that position:—

LOCAL HONORARY SECRETARIES.

Le Commandeur E. D'AMICO, Director-General of the Italian Telegraphs, Rome	ITALY.
J. M. COLLETTE, Engineer of the Netherlands Telegraphs, The Hague	NETHERLANDS.
FRÉDÉRIC DELARGE, Engineer of the Belgian Telegraphs, Brussels	BELGIUM.

C. L. Madsen, Great Northern Telegraph Company, Copenhagen	DENMARK.
C. Nielsen, Director-General of the Norwegian Telegraphs, Christiania	NORWAY.
Don Ramon Pias Director-General of the Chilian Telegraphs, Santiago	CHILI.
Charles Burton, Director-General of the Argentine Telegraphs, Buenos Ayres	ARGENTINE REPUBLIC.
Colonel D. Robinson, R.E., Director-General of India Telegraphs, Calcutta	INDIA
W. E. Ayrton, Professor of Natural Philosophy, Imperial College, Yokohama, Japan	JAPAN.

The President found himself unable, on account of absence abroad on official duties, to give the annual *soirée* at the usual time of year. It was consequently held late in the year, and has been generally characterised as one of the most successful scientific *soirées* ever known. By the kindness of the Council of King's College, the libraries, laboratories, and museum of the College were placed at the disposal of the President; and by the cordial co-operation of Members of the Society, and of gentlemen outside the Society but who are connected with science, an exhibit of instruments and apparatus of high scientific merit was obtained, many of great novelty and utility. The President desires to tender his thanks to all those who contributed so well to the success of the evening.

During the year two numbers of the Journal have appeared; the

third, completing the last session, is now in print, and will shortly be published.

Ths following gives a *resumé* of the various Meetings held throughout the year:—

JANUARY 14.—President's Inaugural Address.

JANUARY 28.—Adjourned Discussion on Mr. G. E. PREECE's Paper on "Underground Telegraphs."

"An Attempt at a familiar Explanation of the Duplex Principle," by R. S. CULLEY, V.P.

FEBRUARY 11.—"On the Application of Electricity as a means of Defence in Naval and Military Warfare," by NATHANIEL HOLMES.

FEBRUARY 25.—"On Military Torpedo Defence," by NATHANIEL HOLMES.

MARCH 11.—"On an improved Double-current Telegraph Key," by J. J. FAHIE.

"On Mr. Latimer Clark's Method of Measuring Differences of Electric Potential," by Professor ADAMS, F.R.S.

"Condensers in connection with Duplex Telegraphy," by R. S. CULLEY.

MARCH 25.—"On the Decay and Preservation of Telegraph Poles," by WILLIAM LANGDON.

APRIL 22.—"Deep-Sea Sounding for Telegraph and other purposes by the use of Pianoforte Wire," by Sir W. THOMSON, President.

MAY 15.—Discussion on Mr. Langdon's Paper "On the Decay and Preservation of Telegraph Poles."

"On the Change of Resistance of High Tension Fuzes at the moment of Firing," by Major MALCOLM, R.E.

"Notes on Electric Fuzes," by Professor ABEL, F.R.S.

NOVEMBER 11.—"Faults in Submarine Telegraph Cables," by J. J. FAHIE.

NOVEMBER 25.—"On Earth Borers for Telegraph Poles," by JOHN GAVEY.

DECEMBER 9.—Annual Report.

"The Telegraph and the Ashantee War," by Lieut. JEKYLL, R.E.

In addition to the above-mentioned papers, the Journal contains a series of original communications, among which are—

"On the Use of Electro-Magnetic Induction in Cable Signalling," by G. K. WINTER, F.R.A.S.

"Indian Telegraph Iron Wire Gauge," by Capt. MALLOCK.

"A Method of Duplex Working," by H. C. MANCE.

"Indian and American Telegraphs," by DAVID BROOKS.

Besides others in course of publication.

The Abstracts and Extracts contain a mass of varied information, both scientific and practical, collected from various sources, and it is intended by the Editing Committee that the useful scope of this portion of the Journal shall be largely increased.

The following have already appeared :—

ABSTRACTS AND EXTRACTS :

"Comptes Rendus," by Dr. PAGET HIGGS.

"On the Magnetisation of Steel," by M. BOUTY.

"Calorific Effects of Magnetism in an Electro-Magnet of several Poles," by M. A. CAZIN.

"On an Electro-Automotor Whistle for Locomotives," by Messrs. LARTIGUE and FORREST.

"Action of the Electric Fluid upon Gases," by M. NEYRENEUF.

"On a new Couple, prepared specially for the Application of Continuous Currents to Therapeutic Purposes," by M. J. MORIN.

"On the Measure of the Electromotive Force of Batteries in absolute units," by M. A. CROVA.

"An Apparatus for Signalling Automatically the presence near a ship of Icebergs," by M. R. F. MICHEL.

"A new Thermo-electric Pile," by M. CLAMOND.

"On Chemical Dynamics," by M. BECQUEREL.

"On the Elementary Law of Electro-Dynamic Action," by M. MOUTIER.

"On the Depth of a Magnetised Stratum in a Steel Bar," by M. JAMIN.

"On the Determination of Simple Substances by the Action of Battery Currents in the Voltameter," by M. E. MARTIN.

"On the Mean Section, Polar Surfaces, and Armatures of Magnets," by M. JAMIN.

"On Electric Chronographs," by M. MARCEL DEPREZ.

"On Phenomena of Static Induction produced with the Ruhmkorff Coil," by M. E. BICHAT.

"On Electro-Static Phenomena in Batteries," by M. ALFRED ANGOT.

"Researches on Electric Transmission by Ligneous Substances," by the COUNT DU MONCEL.

Ditto. No. 2.

"On the Action of two Current Elements," by M. T. BERTRAND.

"Geometrical Illustrations of Ohm's Law," by Professor G. C. FOSTER.

"Argentine Telegraphs."

"The Electromotograph," by T. A. EDISON.

"On the Fall in Pitch of Strained Wires through which a Galvanic Current is passing," by Dr. W. H. STONE.

"On certain remarkable Molecular Changes occurring in Iron Wire at a low red heat," by W. F. BARRETT, F.C.S.

"On the Relationship of the Magnetic Metals," by W. F. BARRETT, F.C.S.

"On Earth Currents," by L. SCHWENDLER.

Abstracts from the various scientific Journals are in the press, contributed by various Members of the Society, who have been aiding the Committee in this useful work. It is to be hoped that ultimately this section of the Journal will contain abstracts and *resumés* of all the Electrical and Telegraphic articles which may appear in the home and scientific journals of the period.

The attention of Members has been frequently called to the necessity of their contributing Papers and Communications for the benefit of the Society, and recently the following circular was issued:—

SOCIETY OF TELEGRAPH ENGINEERS,
4, Broad Sanctuary.

The Secretary is desired to bring before the notice of Members and Associates that the Society's usual Meetings will be resumed early in November, and that these Meetings are not held fortnightly, but on the second and fourth Wednesday in the month, due notice being given of the exact days of meeting.

It is desirable that Members who are willing to contribute papers to be read during the ensuing Meetings should forward them to the Secretary at an early date, in order that they may be approved by the Council, and, when deemed necessary, printed for distribution, in order to invite discussion.

The Secretary is also desired to call the attention of Members to the subject of "Papers" and "Communications," and to invite Members to forward articles of both classes, in order that the interest of the Society and its Members may be maintained. "Papers" are deemed to be Original Communications of such interest as are worthy to be read before the Meetings, in order that the special points referred to in the Paper may be brought before the Members and discussed, so that the progress of Telegraphy may be advanced. These Papers, with their discussions, will be subsequently published in the Journal.

"Communications" may be termed short Papers on various new and interesting points which frequently arise in the every-day working of the Telegraph, and which it is not only interesting but important that the general body of Members should know. Various forms of Communications may be seen in the Journal. And the attention of Members is specially invited to the forwarding of such Communications, for it may be safely relied upon that the frequent publication—whether before the Meetings or in the Journal—of points new to Members which have been elucidated, will be of most material value to all Telegraph Engineers and to the progress of Telegraphy.

Such Communications will be read before the Meetings as may be deemed desirable, and subsequently printed in the Journal.

A list of subjects on which it is desirable that Papers should be contributed is attached.

GEO. E. PREECE, Secretary.

November 2nd, 1874.

SUBJECTS FOR PAPERS AND COMMUNICATIONS.

On the Action of Radiation on the Electric Conductivity of Selenium.

On Lightning and Lightning Protectors.

On Duplex Telegraphy.

On Duplex Telegraphy as applied to Submarine Cables.

On the Action of Marine Life on Submarine Cables.

On Surveying and Dredging to find Proper Routes for Submarine Cables.

On the Durability of Submarine Cables.

On Machinery for the Submersion and Recovery of Submarine Cables.

On Cables for Rivers, Canals, &c.

On the Localisation of very Minute Faults in considerable lengths of Submarine Cables.

On the Manufacture of Iron Wire for Telegraph Purposes.

On the Preservation of Iron Wire in the vicinity of Towns and Manufactories.

On the Gauge of Wire for Long Circuits, and the advantage of using Wire of a higher Conducting Material than Iron.

On the Manufacture of Gutta Percha for Insulating Purposes.

On the Manufacture of India Rubber for Insulating Purposes.

On the Change of Insulation of Material, whether Gutta Percha or India Rubber, with Change of Temperature and Climate.

On the Residual Charge or Polarization of Insulating Materials

On Electrometers, specially with regard to a Cheap Form of Electrometer.

On the best System of laying Underground Wires.

On the Maintenance of Underground Telegraphs.

On Aerial Cables, and their Insulation.

On making Joints in Underground Insulated Wires, where a large Number have to be dealt with in the open Air.

On the Effect of Residual Magnetism, and of "extra" Currents on Automatic Telegraphy.

On Automatic and Fast-speed Telegraphy.

On the Relative degree of Accuracy to be expected from Sound and Sight-reading (Morse), and on the Value of the Morse Ribbon as a record in Cases in Error.

On Earth Currents, and the best mode of observing them.

On Earth Plates in dry places and rocky localities.

On Experience and Experiments bearing on the relative Constancy and Working Properties of different Batteries.

On the Theory of the Galvanic Battery; with a Critical Review of the Evidence for and against the "Contact" and "Chemical" theories.

On Electric Torpedoes.

On Electric Exploders.

On Induction Machines for the Production of Light.

On Insulators of extreme perfection for Indoor Work and for Laboratory Experiments.

On Induction in Overground Wires.

On Line Insulators.

On the Use of Living Trees as Supports for Telegraph Wire.

On Geometrical Illustrations of Electrical Laws.

On Field and Military Telegraphy.

On Pneumatic Tubes as a Mode of transmitting Telegrams.

On Condensers of a Permanent Character.

On Electricity as an Aid to Meteorology.

On Electric Signalling on Railways and in Trains in Motion.

Too much stress cannot be laid upon the importance of such communications, as they are absolutely essential to the vitality of the Society. It must be borne in mind that those Members who

are resident abroad—and their number is great and rapidly increasing—can only have their interest maintained in the Society by the excellence of the Journal, for by that means they are kept *au courant* as to the progress of the Society itself, but more especially as to the progress of electricity and telegraphy; and it is the aim of the Council never to relax their efforts until the Journal has become as complete and as perfect a source of knowledge and information as can be possibly accomplished.

Now that the Society has obtained offices, which draw from the revenue a large amount of money, it has been felt incumbent upon them to bring before the Members individually the necessity of a greater support to the "Publishing Fund;" so, shortly, a circular will be issued to Members inviting their increased support and hearty co-operation. Without such assistance, the Council feel that they will be unable to make the Journal what they wish,—a standard work, forming a most complete and reliable source of information for the guidance of every Telegraph Engineer, Electrician, and Administrator, regarding authenticated progress in telegraphic science. Abundant testimony has been already received, both at home and abroad, of the value of the Journal, and it is to be trusted that that testimony will ever be on the increase.

The remarks of the President upon the important question of "Terrestrial Electricity" have opened up a wide field for research, and assistance has been offered to the Society from many directions to aid in the elucidation of this question by the periodical observation of "earth currents." By a continued and careful series of observations, taken from different parts of the world, results may be finally obtained which will throw immediate light on this abstruse problem; and, with the view of obtaining united action, a Committee has been formed for deciding upon a uniform system. Your Council hope that, with the advice and assistance of the President, Sir William Thomson, a definite code of rules may be decided upon, which, being issued to the Members of the Society scattered over the world, will at once establish such a body of observers that only a Society such as the Telegraph Engineers could possibly accomplish, numbering as it does such a vast quantity of skilled scientific observers.

The President, in resigning his functions, has great pleasure in

SOCIETY OF TELE

STATEMENT OF RECEIPTS

For the Year ended

RECEIPTS.

		£	s.	d.	£	s.	d.
By Balance Cr. 31st Decr. 1873	... ...				361	18	10
„ Subscriptions 1872	... ...	10	9	0			
„ „ 1873	... ...	70	2	0			
„ „ 1874	... ...	465	6	6			
„ „ 1875	... ...	14	8	0			
„ „ 1876	... ...	3	0	0			
„ „ 1877	... ...	1	0	0			
					564	5	6
„ Subscriptions to Publishing Fund 1872	... ...	8	15	6			
„ „ „ 1873	... ...	16	16	6			
„ „ „ 1874	... ...	12	13	0			
					38	5	0
„ Commutation of Life Subscriptions ...	... ...				31	0	0
„ Sale of Journals	... ...				38	7	4
„ Gain on Exchange	... ...				0	0	3
		Total			£1,033	16	11

ASSETS OF THE SOCIETY ON THE 31ST DECEMBER 1874.

	£	s.	d.
Cash Balance	62	4	10
Arrears of Subscriptions	352	8	6
Furniture	253	8	0
Value of Stock of Journals	332	0	0
	£1,000	1	4

STATEMENT OF

On 31*st*

RECEIPTS.

	£	s.	d.
By Donations	200	0	0
„ Compositions of Life Subscriptions	109	15	0
Total	£309	15	0

GRAPH ENGINEERS.

AND EXPENDITURE.

31*st* *December*, 1874.

EXPENDITURE.

	£	s.	d.
To Salaries	185	0	0
„ Shorthand Reporter	21	10	6
„ Attendance and Refreshments	19	2	11
„ Furniture	241	18	0
„ Printing and Stationery	210	5	7
„ Advertising	1	11	0
„ Rent and Taxes	230	0	0
„ Petty Expenses, including Postage	58	1	7
„ Overpaid Subscriptions refunded	4	2	0
„ Loss on Exchange	0	0	6
Total	£971	12	1
Balance Cr.	62	4	10
	£1,033	16	11

(Signed) C. E. WEBBER, MAJOR, R.E., *Treasurer.*

We have compared the above Account with the Vouchers and Cash Book, and find it to be correct, leaving in the hands of the Treasurer sixty-two pounds, four shillings and ten pence.

(Signed) J. WAGSTAFF BLUNDELL, FRED. CHAS. DANVERS, } *Auditors.*

GEO. E. PREECE,
Secretary.

CAPITAL ACCOUNT,

December, 1874.

EXPENDITURE.

	£	s.	d.
To Furniture	253	8	0
Balance Cr.	56	7	0
	£309	15	0

announcing to the Members that Mr. Latimer Clark has accepted his nomination to that office for the ensuing year; and, from the well-known practical experience and scientific attainments of the President elect, and from his frequent and active attention to the interests of the Society, he feels sure that its status will be still further improved, and that the prosperity which has attended it will continue, so that our Society, which has already taken up so high a position in the scientific world, will, by his presence and exertions, be raised still higher.

The Council congratulates the Society that the hopes which they expressed in the closing paragraph of last year's Report have been more than fulfilled, and at the next Annual Meeting they trust to meet the Members with the assurance that the Society has still further enlarged its scope for utility for the general benefit of applied science, and for the universal progress of Telegraphy.

The CHAIRMAN: Before we proceed further with the business, it is only becoming on our part to take some notice of the very sad loss the Society has sustained and the sad bereavement which has occurred in the recent loss of the vessel "La Plata." I do not feel so much sympathy for the gallant fellows who have gone down in her, because, like soldiers, they have died in the discharge of their duty; but we must feel the deepest sympathy for those who are left behind, and especially for the relatives of our six professional brethren who have lost their lives in this sad catastrophe. One of them was a member of this Society, another I am sorry to say was a relative of our President. It is the duty of every Member of this Society to do his utmost to aid in the subscriptions which are being raised for the benefit of the families thus suddenly bereaved. There are many vacant chairs at the fireside to-night, and, considering that we as members of the same profession may at any time be called upon to follow in their footsteps, it is at once our interest and our duty to help to the best of our power those who are left behind. It is now my duty to move that the Report just read be adopted, and printed for circulation among the Members.

The Resolution was unanimously adopted.

The following paper was then read:—

THE TELEGRAPH AND THE ASHANTEE WAR.

By Lieut. JEKYLL, R.E.

Before commencing my narrative of the Construction and Working of the Electric Telegraph during the late campaign, it will be necessary to sketch very briefly some of the earlier events, in order to render intelligible what follows, and, for the same reason, I shall have to allude in the course of my remarks to concurrent events, which, although in themselves foreign to my subject, yet have so direct a bearing upon it, that it will be less confusing to mention than to omit them.

As you are doubtless aware it was originally intended, in view of the notorious deadliness of the West African climate, to carry on the war by means of native levies, led by English officers alone, raised from among the friendly tribes of the Coast, and it was only in the event of the failure of this desirable end that the employment of white troops was contemplated; but, as it appeared probable from the commencement that white troops would eventually be needed, every precaution was adopted to secure for them a period of service on the Coast as short as was consistent with the complete attainment of the objects of the war.

Partly to carry out these views, and partly to facilitate the transport of munitions, a light field railway was designed, the plant for its construction was rapidly prepared, and shiploads despatched to the Coast as quickly as the materials could be delivered. As every preparation for the war had been carefully and anxiously considered, and every want which experience could suggest had been attended to, it might perhaps be asked how it was that, the use of the telegraph being neither unknown nor novel in warfare of the present day, no provision for this means of communication was made at the commencement of the campaign.

I believe I am right in saying that the question was discussed, and decided in the negative, in expectation of the facilities which were anticipated from the railway, as well as from the desire of reducing to the utmost the number of Europeans to be sent to the Coast.

The General and the small band of officers who accompanied him reached Cape Coast Castle early in October. A few days sufficed to show that the railway was impossible from the nature of the country, a few more proved as conclusively that the idea of native levies was equally impossible from the nature of the people. Within a week despatches were sent home which brought out three white regiments, stopped the railway, and in its place ordered a line of telegraph.

On the receipt of the demand for the telegraph, measures were at once taken to provide a suitable equipment. A large quantity of stores were ordered from manufacturers, and a detachment of twenty-five non-commissioned officers and sappers skilled in telegraph work ordered to join the 28th Company of Royal Engineers, which was about to embark on board H.M.S. Himalaya for conveyance to the Coast.

So short, however, was the notice, that the supply of stores could not be delivered in time to accompany the troops, and hence it was evident that the detachment, on reaching its destination, would be kept in idleness pending the arrival of stores. This was foreseen by Major Webber, who, by extraordinary exertions, and with the concurrence and assistance of the General Post Office, collected a supply of stores equivalent to about 30 miles of line, together with two direct inkers and two 10-cell Leclanché batteries. These were forwarded from various depôts by passenger and even mail trains to Plymouth, where they were taken on board the Himalaya and stowed in the officers' baggage-room.

The telegraph detachment of 25 men consisted of non-commissioned officers and sappers who, for three years or upwards, had been engaged exclusively in telegraph work in the service of the General Post Office. Six were experienced clerks, the

remainder construction-and-maintenance-men. All had a thorough practical knowledge of the technicalities of the work.

We left Plymouth on the 19th November, and, embarking the 2nd Batallion Rifle Brigade at Queenstown, continued our voyage to Cape Coast Castle, where we arrived on the evening of the 9th December, 1873.

To return to the operations on the coast itself. The demand for white troops having been supplied, preliminary operations were begun on a small scale, commencing with the destruction of some villages near Elmina, which harboured Ashantee troops. This was shortly followed by the break-up of the great Ashantee camp at Mampon, which up to that time had been a standing menace to Cape Coast Castle, and the retreat of the main body of Ashantees northwards towards the Prah. This period of the campaign culminated in the battle of Abracrampa early in November, by which the retreat of the Ashantees was converted into a rout, and they were forced with all speed to evacuate the Protectorate and cross the Prah into their own country.

Meanwhile other operations less demonstrative but no less important were in progress. The road up to the Prah—that great work upon which the chance of success of the expedition entirely depended—was rapidly advancing. First to Dunquah, 20 miles from Cape Coast, then to Mansu, 18 miles further still, through mile after mile of dense tangled forest and pestilential swamp, the head of the road moved steadily forward. At intervals of from seven to ten miles clearings were made and standing camps of substantial and orderly huts constructed for the convenience of the troops, and arrangements for a supply of good water, stores for food and ammunition, and such comforts as could be provided, were carefully made in an incredibly short space of time. Thus by the time the Ashantees were clear of the Protectorate the road had advanced some fifty miles, and a large body of native labourers and artificers, organised and directed by Colonel Home, R.E., were busily carrying out the engineering works.

Such was the state of affairs when on the 9th December the Himalaya anchored off Cape Coast Castle. After a delay of a day

orders came for the landing of part of the telegraph detachment and a few men of the 28th Company Royal Engineers; the rest were to remain on board with the battalion of the Rifle Brigade and cruise at sea for three weeks.

At this time the plan of the campaign was, on the completion of the road to the Prah, to form a large camp at Prahsu, and there assemble the entire force, with an adequate supply of stores, preparatory to the invasion of Ashantee.

The road and camps being as yet only partially completed, the arrival of the troops was premature, for so promptly had the Government at home complied with the General's demands that the regiments were at Cape Coast three weeks before they were expected.

However, so far as we were concerned, this delay made no difference, so, landing with fifteen of the men and the Post Office stores which we brought out, I was placed by Colonel Home in charge of the telegraph works and ordered to proceed with all possible speed.

Having seen the stores safely landed and housed, I went at once to a place called Beulah, nine miles off, which had been an advanced post, and was now occupied by a tribe of natives. They had been engaged in cutting down a large number of bamboos which grew there in great abundance. Armed with a large bag of silver coins I met the chiefs, and, counting the number of bamboos in possession of each, made a bargain and paid for them on the spot. In this way we acquired in the course of the afternoon about 400 good bamboos, varying in length from 18 to 20 feet, and, though quite green, sufficiently strong to carry a light wire. The next day a party of fifty women was sent out to fetch some of the bamboos in, which they did in the course of the evening.

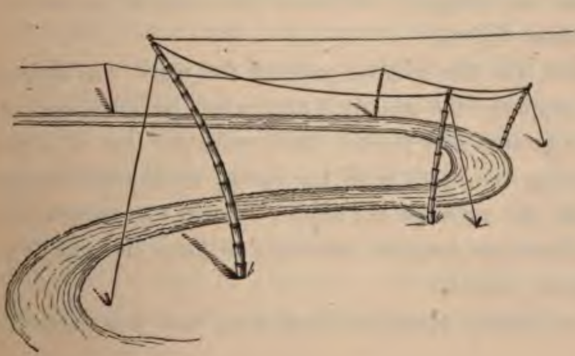
It was now necessary to devise some means of attaching insulators to the bamboos, and this we did in the following manner. Having sawn off the top of the bamboo, square, 6 inches above a joint, a piece of No. 11 wire was lapped four or five times tightly round the top, to prevent it splitting, and a plug of soft wood was then driven in with a mallet, until it completely occupied the hollow of the joint.

An auger-hole was then bored down the middle and the insulator screwed in. This plan answered perfectly, and no insulator fixed in this way became loose while the telegraph remained standing.

We were now furnished with a gang of fifty natives, whom we were to retain permanently—that is if we could. They were not promising in appearance, and I was compelled to dispense with the services of those who were less than 4 feet high. But they had with them an intelligent head man, and by dint of supervision and organisation, supplemented by a little flogging now and then, they turned out a tolerably useful body for light work as niggers go.

They soon learnt to fix the insulators on the bamboos, and in two days fitted up a large number, while some were engaged in burying an earth-plate deep in the ground at Government House.

We now commenced the construction of the line. Beginning at a shackle on the roof of Government House, and passing thence on bamboos through the streets, the wire followed the course of the bush road. Our bamboos were set at distances apart varying from 60 to 90 yards; they were planted two feet deep and blocked with stones at the foot, and at every angle were supported by wire stays secured either to bushes or pickets driven into the ground. Nearly all bamboos have a natural bend, and of this advantage was taken in setting the poles, making the bend oppose the tension of the wire, and at considerable angles, giving the pole a sensible inclination besides.



For the first eight miles the path wound among low but steep hills devoid of timber-trees but covered with thick brushwood. For the most part, our bamboos were tall enough to raise the wire above the growth of shrubs, but at the tops of all the hills were groves of greater height, which could not be so cleared, and whenever these were encountered we were obliged to resort to the axe. At one place the trees met overhead, and the width of the road clearing had to be doubled for a distance of half a mile before sufficient headway for the wire could be gained; and then, owing to the tortuous course of the path, it was necessary to set the poles at very short intervals and on alternate sides, to keep the wire clear of contact. It took a whole day to penetrate this thicket, for, independent of the time occupied in setting so many poles, we were badly off for cutting-tools.

We continued to advance, making progress at the rate of about two miles a day. We should have got on quicker had our native labourers been at all superior to monkeys in intelligence and strength. Their stupidity was equalled only by their aversion for work. It took some days to teach them how to unroll a coil of wire, and ladders were implements which they utterly repudiated. There is, however, this to be said, that the best of the people had been secured before we reached the coast, either for the native regiments, police, or engineer labourers employed upon the road, so that the telegraph gang was by no means a fair sample. In fact these men were little better than the dregs of the Fantee population, and what a depth of debasement is implied by this description none can imagine who have not come in contact with the inferior African tribes.

The result was that the greater part of the work was done by our handful of sappers; the labourers excavated the holes, each man squatted on the ground and pecked feebly with the iron of a pick-axe, and removing the earth with his hands, while some chopped the bushes; but the moment the sapper's back was turned work stopped. We therefore found it necessary to keep the gang in as compact a form as possible.

The wire was simply stretched hand-taut, and was carried up

to the end of the pole-work each day. It was bound in to the insulators with a fine wire in the ordinary way.

In four days we reached the first camp, Inquabim, seven miles from Cape Coast, where we joined up one of our instruments and one of the 10-cell batteries.

A neighbouring swamp to which we ran a bare wire afforded excellent earth, and we exchanged good signals with Government House. Throughout the operations we never failed to find good earth, as there was always a swamp handy to every camp.

It would have been useless to set up a permanent telegraph station so close to the coast, so the instrument remained in use only until the next camp was reached, when it was shifted forward —for at this time we had only the two which we brought with us.

For a mile beyond Inquabim the line was continued on poles as before. But now the face of the country was beginning to change, and here and there huge timber trees broke the monotony of the endless brushwood. By degrees the trees increased in number, and occasionally one by the roadside afforded a point of support to the wire.

We had a supply of small iron brackets which were very easily fastened to trunks of trees by means of three nails, and into the brackets so fixed the porcelain insulators were screwed.

As we advanced the forest thickened, and fewer and fewer poles were needed, till at last the line was carried almost entirely on the brackets, and poles were used only in exceptional places.

Still the rate of progress remained unchanged, for, although the labour of setting and staying poles was dispensed with, there were greater difficulties in the way of running out the wire and threading it between the tree trunks, while the density and luxuriance of the undergrowth and creepers appeared to increase at every mile. Groves of bamboos, too, were occasionally encountered, and these were particularly laborious to cut through.

On reaching the next camp, Accroful, the instrument was brought up, and communication kept up as before, while the line was pushed forward to Dunquah, twenty miles from Cape Coast, an important post and depôt of carriers and stores.

Here our first permanent office was opened. A regular office-hut was built, and two men stationed, one to act as clerk the other as line-man. Both men lived in the hut.

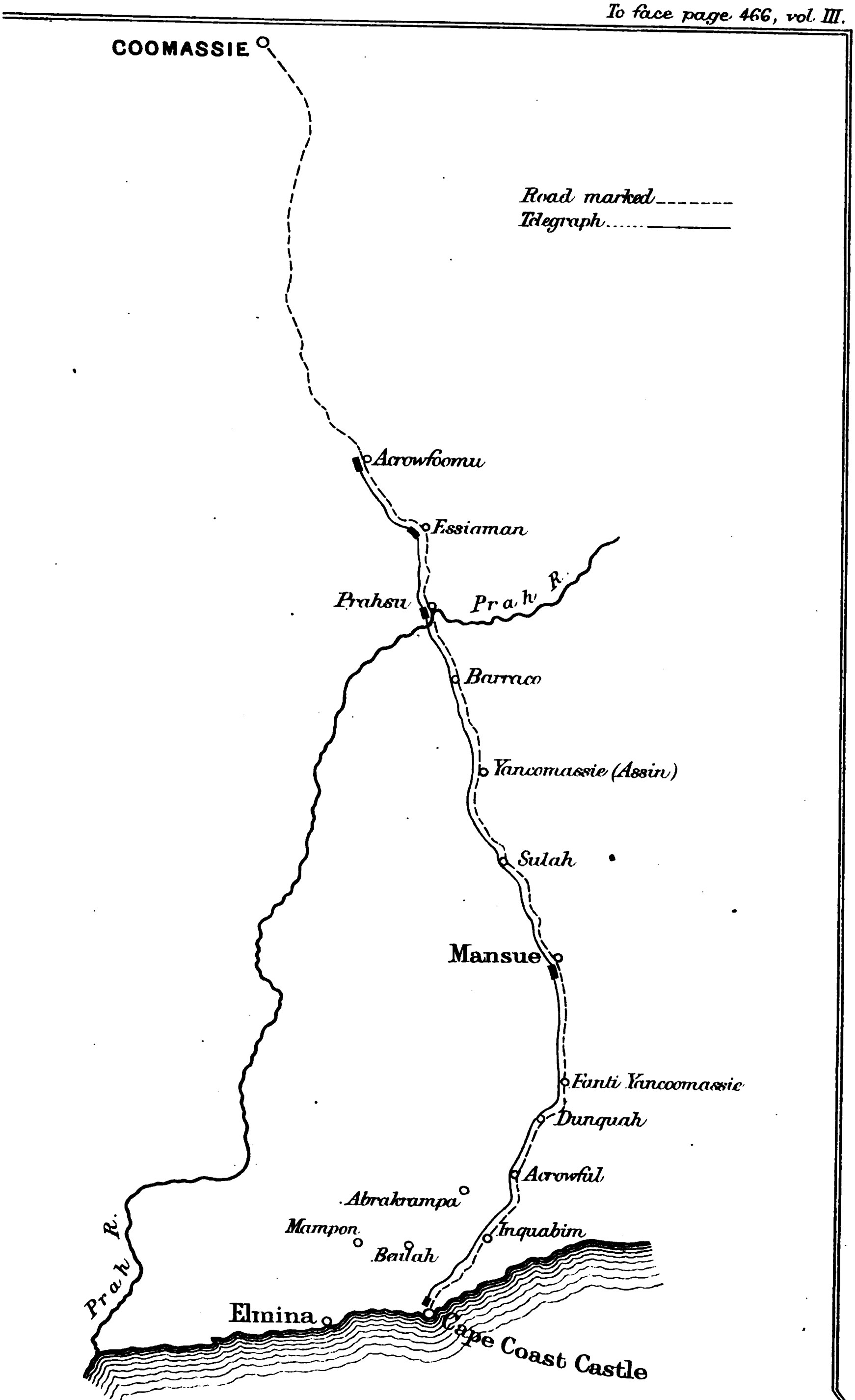
Two clerks had been already stationed at Government House, as the duty there was heavier than at the bush stations, and a line-man had also commenced duty from that end of the line.

It was my intention to post a line-man at every twenty miles, furnishing him with a small gang of four or six native labourers, and a supply of tools. His duty was to patrol his length daily on one side of his head-quarters, and keep the line in order, cutting back the rapid growth of succulent plants, contact with which would have involved a total loss of current.

Up to this time the communication had been twice interrupted, on both occasions by the fall of trees across the wire. It was easy to foresee that this would be our greatest source of trouble as regarded maintenance. By the end of the year we had reached the camp of Yancoomassie (Fanti). Our small stock of stores was just exhausted when the joyful news came by wire that H.M.S. Dromedary, laden with stores, among which was the telegraph equipment, had arrived at Cape Coast. At the same time, also, the troopships returned from their cruise, and preparations were made for the debarcation of the regiments. The road was now completed up to Prahsu, and the great camp was in an advanced state, while stores and ammunition poured in in a regular stream.

I at once hastened down to the coast to see to the landing of our much-needed stores. I found the work already begun and in active progress; there were wire and insulators in abundance, and, what was quite as welcome, a goodly supply of excellent tools, besides instruments and batteries, and hundreds of things which it is unnecessary to enumerate.

Having seen the stores landed, the next thing was to send them up to the working party, who were beginning to languish for want of them as a parched land longs for rain. But I was doomed to disappointment. The troops were landing and marching up the country, and they needed carriers for their baggage; they must be provided for first. " If we have any carriers left after the departure


COOMASSIE
Road marked
Telegraph
Acrowfoomu
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Prahsu
Prah R.
Barraco
Yancomassie (Assin)
Sulah
Mansue
Fanti Yancoomassie
Dunquah
Acrowful
Abrakrampa
Mampon
Bailah
Inquabim
Prah R.
Elmina
Cape Coast Castle
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of this half battalion they shall be at your disposal." Such was the answer to my urgent appeal. To-morrow, perhaps, we shall have a few to spare, but when to-morrow came there were none to spare. And so matters went on for three or four days.

On the fifth day 200 carriers were definitely promised. The stores had been waiting, duly addressed, in loads of 50 lbs. to 60 lbs., ever since they were brought on shore, and the moment the 200 carriers were told off the loads were distributed, and I had the supreme satisfaction of seeing them move off soon after mid-day.

Judge of my surprise and disgust when three hours later I saw the whole number returning, loads and all. It was the day before that fixed for the landing of the 23rd regiment. The carriers for the regimental baggage had been assembled at Dunquah and were coming down to the coast the same evening, when intelligence arrived that the whole body had deserted; this was the cause of the recall of my carriers; they were required to replace the deserters.

The wholesale desertion of carriers was now assuming alarming proportions, and almost threatened to paralyse the operations. The country being cleared of Ashantees, the people knew that they could bolt into the bush with impunity and without fear of pursuit—and numbers took advantage of their knowledge. Of our small gang of telegraph labourers, many disappeared in this way, and we had to replace them as best we could with men and boys picked up at the camps and villages on the road.

Matters soon improved, however, under the energetic action of the officers charged with the duty of obtaining men, and the movement of troops continued without further interruption.

After a delay of seven days, a large supply of telegraph stores was at length sent up to Mansu, and in the course of a few days more enough had been deposited at the different camps to provide for the completion of the line to Prahsu.

During this irritating delay I had succeeded in obtaining a few men by sending the best of our labourers through the town to collect as many as they could catch, rewarding them by giving them charge of the gangs they raised, at a high rate of pay. So

in the course of this week three small parties of twenty each were despatched to the front with such stores as they could carry, by which means the working party was just kept going.

My detachment of Sappers was now raised to its full strength of twenty-five by the addition of the ten men who up to this time had remained on board ship. All now joined the working party except those permanently stationed in the town and at Dunquah, and the rate of advance was somewhat increased, being about three miles a-day.

But now the exposure and hard work began to tell upon the men. Two were already disabled from fever; and all the others suffered more or less from attacks of short duration.

At Mansu, a very important post, thirty-six miles from Cape Coast and sixteen from Dunquah, our second permanent office was opened, and we worked from here to Cape Coast, with Dunquah intermediate. A line-man was also posted here. From this point the line advanced successively to Sutah, Yancoomassie Assin, where another line-man was stationed, Barraco, and Prahsu. Communication being opened as each camp was reached, signals in all cases were good, and, with the exception of a few faults due to falling trees, free from interruption. The office at Prahsu was opened about the 20th January, and the line, seventy-five miles in length, was working with four offices in circuit.

During the construction of the line beyond Mansu a tolerably uniform distribution of the working party was adhered to.

First went two experienced men with a few labourers, selecting trees suitable for attachments and fixing the insulators. Then came an axe-party of forty, who cleared away the undergrowth and creepers to an extent sufficient to permit of the wire being run. Then the wire itself was run out, stretched, and bound in, and finally another axe-party removed trees which either were or threatened to be in contact with it.

The selection of points of attachment demanded no small amount of discrimination. I made it a rule to avoid all medium-sized trees. Very large trunks were good, as no perceptible motion of swaying was to be anticipated at the height at which the wire was fixed.

Very small ones were equally good, because their tops could be cut off immediately above the point of attachment, which rendered them steady.

Some trees, otherwise suitable, had to be passed by on account of the mass of tangled creepers with which their trunks were encumbered; some, owing to ants or other poisonous insects with which they were infested.

The greatest pains were taken to make the line as secure as possible, and to guard against preventible causes of failure. The staff of workmen was so small, and the distance to be covered so great, that I foresaw that a breakdown of any extent would be attended with delay disastrous to the advance, so that, though by adopting a less careful system a quicker rate of progress might have been achieved, it would have been at the expense of safety and ultimate progress.

And here I venture to express an opinion that the most important attribute of a military line of telegraph is reliability. There is still a strong prejudice in the minds of many military men against telegraphs. By such, the applications of modern science to warfare are regarded with a species of hostile awe. It is important that the grounds for this prejudice should be removed, which can only be done, in the case of the telegraph, by showing that it is reliable. The question most commonly asked at a military telegraph-office is, "Can you send a message to such and such a place?" Hence the principle to which, above all others, I clung, was, within the range of possible precaution, to take such measures as would render this inquiry superfluous.

When the line reached Prahsu, I was in hospital at Cape Coast, utterly prostrated with repeated attacks of fever, and a few days later I was ordered home.

Meanwhile the troops had entered the kingdom of Ashantee, and it was currently reported at Cape Coast that the war was over. The king was said to have acquiesced in the conditions imposed upon him, and the regiments were about to return to the coast. However, I had orders to push on the line beyond the Prah, and its construction was continued as before. An office was opened at

Essiaman, and afterwards at Accrofumu. This was the furthest point reached, being slightly over 100 miles from Cape Coast, and there were at the same time four other offices in circuit, all working well. But on attaining this place bad weather began to set in. Numbers of trees fell across the line south of the Prah, and it was found necessary to withdraw the whole working party to make good the damaged portions. By the time this was done Coomassie had been taken, and the campaign was virtually at an end. The forces returned as quickly as possible to the coast, and the instruments and batteries were removed as the necessity for telegraph stations ceased. The line remained standing.

It is worth remarking that the whole of the interruptions were due either to falling trees or to the failure of ebonite insulators, the special insulators sent out being of that material. Whether they failed owing to undue weakness in construction, or to the effect of heat upon the material, I am unable to say. From whatever cause, a great number of cups split and fell off in two pieces, or became detached from the shanks, in either case permitting the wire to fall to the ground. The pole line and porcelain insulators stood well to the last, the bamboos being particularly satisfactory. They had become seasoned while standing in the ground, and in moist places some were beginning to grow; but not one failed.

It is satisfactory to know that where bamboos can be procured they may be trusted implicitly to carry a single wire. For this purpose a bamboo is perfection, combining strength with lightness to a degree unattainable with any other material. I am not so clear by what means it would be possible to attach more than one wire, and this it would be valuable to know. An earth-borer of five inches diameter would be of great value in constructing a bamboo line.

We never experienced an instance of malicious damage by the natives. They regarded the telegraph as white man's fetish, and held it in wholesome respect as such. The Ashantees had evidently heard tidings of it, and looked upon it as a powerful charm; for shortly after crossing the Prah the advanced parties discovered a white cotton thread suspended from the trees by the roadside for a

distance of many miles, obviously in imitation of our wire. The sensations of respect on the part of our own labourers was considerably heightened by receiving some severe shocks of lightning while handling the wire, and at one time I feared we should suffer serious delay from this cause. Our instruments were all furnished with lightning protectors, and none of the coils suffered, though on one occasion the line-wire was fuzed.

Before concluding I cannot resist mentioning some points of general bearing, which have been suggested by the experience of the Gold Coast.

First, as to materials--the most important of these is the wire. The description which we used was ordinary No. 11 galvanized iron, and it certainly is excellent for the purpose. At this moment, were I appointed to take charge of telegraphs in the field, I should certainly select No. 11 wire. Besides its special use as telegraph wire, it is of great value in many descriptions of engineer work. The only thing to be said against it is its weight—viz. 2 cwt. to the mile. A strand wire of 3 No. 18's, twisted, would be nearly as good and ½ cwt. lighter. Of the various new inventions, such as steel and copper compound wire, some may be more suitable than anything hitherto used, but I should be sorry to trust to anything which had not been fully and fairly tried. I purposely exclude all mention of covered wire, of which we had some coils, but which were too heavy to be carried.

Next as regards insulators. My faith in ebonite has been shaken, and I think porcelain far preferable. The light S. S. insulator used by the Post Office behaved as well as possible. It might perhaps be made lighter, though I should be loth to acquire this advantage at the expense of adequate strength.

The direct Morse instruments are well suited for military work, and so also would sounders be, while Leclanché batteries leave nothing to be desired.

As regards men: the clerks must be as good as possible, and must have such a knowledge of engineering work as will enable them to rectify faults which occur in their own offices, and to localize faults outside,

Our clerks were very hard worked. Each man had charge of an office night and day, sleeping by his instrument, and liable to be roused at any hour. This was too much, and points to the necessity of a considerable number of trained military clerks. Line-men must be thoroughly well-trained in detecting and remedying faults; they will often have to act under great responsibility, and will be deprived of many facilities for localization.

I have felt some reluctance in addressing this meeting to-night. It seems scarcely worth while to occupy the time of such a number of eminent scientific men with the description of so trifling a work as the erection of 100 miles of single wire through a wood. But I am emboldened to address you by the consciousness of the great and growing importance of telegraphy in war, and in the hopes of hearing a discussion on a subject of such deep interest to a military engineer. Military telegraphy is not yet as perfect as we hope it will be soon, and as the experience of Members of this Society will help to make it. The suggestions and experience of practical men, gathered in all parts of the world, and under all circumstances of climate and locality, will materially advance the science of telegraphy as applied to war, and these it is which the Members of this Society are peculiarly qualified to give.

Mr. Bell asked whether the bamboos were sufficiently strong to admit of ladders being placed against them in the erecting of the wire?

Lieut. Jekyll replied, that step ladders were used, which did not touch the bamboos at all.

Lieut.-Col. Home, R.E., said: As I commanded the Royal Engineers during the Ashantee Expedition, perhaps you will not object to hear a few words from me. Before making any remarks upon the telegraph and the plan which Lieut. Jekyll has told you of, I would make a few remarks on what he has not told you of; and rather deal with sins of omission than those of commission. I should like to say, in the first place, how admirably he himself worked—how he watched the progress of his wire, and how everything connected with this telegraph in Ashantee was attended to by him. I feel especial pride in saying this, not only as a brother officer, but as a

member of this Society, because I consider there should be *esprit de corps* amongst telegraphists, whether military or civil; and thus in the Ashantee campaign; whilst to those who, like myself, went in at one end of the country and through it to the other there was a certain amount of pleasure and reward to be obtained, in the case of Lieut. Jekyll he had the heavy drudgery with little reward—a good deal of hard work combined with the exercise of a considerable amount of skill. To him and to other officers in the Post Office Telegraph Staff I feel too much credit cannot be given. When, after the capture of Coomassie, I was returning to the coast, I was told by an officer that on some occasions a clerk was called away from one station to go to another, and was afterwards sent back to his former station. In the meantime a black man had to watch the instrument, and when the clerk came back he read the message off and sent it where it was meant to go to. This is a thing which as telegraphists we may be proud of. I think one of the advantages of this Society, to ourselves and others, is the giving of useful advice to one another. In countries where universal service prevails, the army commands the admiration of the country, and the service is made complete in itself; but in this country the army expects, and has always found, assistance from its civil brethren.

There are certain points of telegraphy which I should like to mention, in order to centralise the discussion and not let it wander off into vague channels. What are the functions of a military telegraph? The functions of the telegraph in a European war in a temperate climate are divided into three distinct heads. There is the telegraph which connects the *corps* of an army; then there is the telegraph which unites the various moving portions of an army. That must be essentially a military organisation. We must have the means of rapidly laying down and taking up the wire; and the next which follows in the rear of that must be more or less military in its organisation. But the vast mass of the telegraph of the army may or may not be military, and would be, in case this country took the field, almost entirely a civil service, because in case of war we should be obliged to call upon the Post Office Telegraph service to supply clerks to be employed in the

rear of the army. Whether the telegraph material itself be good or bad, that is the material we must use, and on which we can lay our hands in large quantities—materials which a large number of people can handle, and which, being articles of store and trade in the country, you can avail yourself of at once. Then with regard to the advantage of the telegraph service of this country being in the hands of the Post Office Government authorities, it was strikingly manifested in the instance of the expedition to the Gold Coast. The promptitude and despatch with which immense quantities of telegraph wire, with the necessary instruments and all other matters, were put on board the "Himalaya" were, I may say, unrivalled. Whether Mr. Scudamore, or Mr. Culley, or Major Webber is to be thanked for that I do not know; but the rapidity with which the materials were collected together and put on board ship deserves to be placed on record.

In addition to the various kinds of telegraphs I have enumerated, there is another, as compared with that employed on the Gold Coast. In the event of the expedition landing from the Persian Gulf and advancing to Herat, a special class of telegraph would be required, and to that I would direct attention for a moment. This may be divided into four heads: viz., the wire, the insulators, the poles, and the instruments. The wire Lieut. Jekyll has told us of, for my part, I think too heavy. In all these expeditions transport is the great difficulty. The difficulty of the telegraph on the Gold Coast was the transport. We had an immense number of labourers employed, but they were engaged in bringing up food and ammunition, and other articles essential in war. If we had had a wire weighing about 1 cwt. per mile, instead of one weighing over 2 cwt. no doubt the thing would have gone on faster; and I think the question of lighter wire is one of the highest importance. A quantity of compound steel wire was taken out, but Lieut. Jekyll did not like to use it, and I agree with him in his reason for that. Like as David would not use Saul's armour because he had not proved it, so I think nothing in these cases should be used as experiment. Consequently, I think the question, whether a light copper wire was likely to have succeeded in this telegraph, is one to which the

attention of the meeting might be directed. Then, with regard to the insulators, it has been said they were not successful; but on my return I made a close examination of the whole line. I think I looked at every insulator, and in the whole length of 104 miles there was, I think, only one damaged insulator. That speaks volumes—not only for the insulators themselves, but for the care exercised in the construction of the line. It may be that some of the insulators were fractured from being too hastily made by whoever manufactured them. The style of insulator I should prefer for work of this kind would be one which could be fixed up by unskilled workmen, and afterwards finished off by skilled workmen.

With regard to instruments, two years ago, I had a great argument with Mr. Preece on the subject of instruments. He was in favour of the sounder, while I was, and am now, in favour of the recorder. A sounder sounds its messages better and will work very fast, which is a matter of consideration when a telegraph is worked for the purpose of producing revenue. But a military telegraph does not work for revenue, although one message sent by it may be worth £7,000 or £8,000. One message sent from Cape Coast to Gibraltar was done at a cost of £5,000. When you send messages like that, revenue is not a thing to be thought of. One advantage of the recorder is, you have an absolute record of what passes over the instrument; and, in the case of the message to which I have just referred, I had three positive proofs of the message having been correctly sent. Now, to a person in charge of the working of a telegraph it is an enormous advantage for him to be able to say, "Here is what the instrument said," and of the value of that in military messages there can hardly be any discussion. It is further to be considered that where these expeditions have been made those engaged in them are greatly exposed to fever. In Abyssinia, on the Gold Coast, and in New Zealand, where these lines have been made, fever is the great enemy. One of the remedies administered in cases of fever is quinine, and one of the effects of quinine is to produce deafness. On one occasion I recollect going to the station to send a message by telegraph, and when I spoke to the man he shook his head and said I must

write it down. He was deaf from the effects of quinine, and it would therefore have been impossible for him to have worked with a sounder. We were short of clerks at the time, and occasionally there was no one to receive the messages at the time they were sent; the great advantages of the recorder instrument were therefore manifest. Whilst the clerk was laid in bed the native in attendance was trained sufficiently to be able to take the slips off the instrument.

With regard to poles, in Asia we had both bamboo and trees. The latter worked very well indeed. I had doubts whether they would answer, but the results of a small piece of line I made at first led me to recommend their adoption. There would be no difficulty in attaching more than one wire to bamboo poles by putting a clip on the top screwed on by means of a collar. If the meeting considers these four subjects—the wire, the insulators, the instruments, and the poles, which would be best for use in a country where neither bamboos nor timber exist—if the discussion were confined to those points--some useful results might be arrived at.

Major BATEMAN CHAMPAIN, R.E.: I am not going to detain you with what I might myself have to say on this subject. I did intend to address you, but only in the place of a gentleman who was not present at the beginning of the evening, but whom I am now happy to see here. I allude to Major St. John, who had charge of the line of telegraphs used in the Abyssinian Campaign, which was not altogether unlike the one described by Lieut. Jekyll. Major St. John can tell you the history of that line. I would just say to Colonel Home, that, whenever a military telegraph may be required in connection with Herat, we shall be happy to help him with a permanent line for a great part of the distance.

Major ST. JOHN, R.E.: I am sorry I arrived here too late to hear Lieutenat Jekyll's paper. I heard some of Colonel Home's remarks, and I have previously had the pleasure of reading a communication of Lieutenant Jekyll's, on the subject of military telegraphy, in the Engineering Journal published at Chatham. In that I noticed that the most prominent defects in the materials supplied to them, by what department of the Government I do not know,

were precisely the same that I myself experienced in the materials provided for the use of the Abyssinian Expedition. In those days military telegraphs were in their infancy, and all we had to go upon was some vague experience of the mode adopted by the Prussian Army in the Sadowa Campaign—experiences valuable in themselves, although they were but little applicable to the exigencies of an African campaign. The bracket insulator which Lieutenant Jekyll found so useful failed occasionally. The spike insulators were perfectly useless. I have no doubt in the Ashantee campaign, as in the Abyssinian, we both equally suffered from want of sufficient means of transport. As far as I was concerned, we had no native labour, whereas Lieutenant Jekyll had the benefit of that, such as it was. I had only a few soldiers here and there, and I had no timber of any kind whatever, but was entirely dependent upon bamboos, which were sent from Bombay, and were ordered in lengths of 18 feet. The ingenious people to whom the order was given however thought it would be inconvenient to have lengths of 18 feet carried by camels, and therefore they sent them in two lengths, to be fitted together with iron sockets, but they forgot to take the precaution of tying the pairs of bamboo together, so that when they arrived it was impossible to put the bamboos into their fellow-pieces, and they had to be used either in single pieces or tied together. I found insulators quite unnecessary with bamboos. The plan I adopted was to cut slits about six inches across the tops, place the wire in them, and take two or three turns round the head of every few poles.

The CHAIRMAN: In what length of line did that plan answer?

Major ST. JOHN: I had no difficulty in working without relay 100 miles. The bamboos were old and seasoned, and I think they made quite as good insulators as porcelain as long as rain was not actually running down them. The wire which I took out was No. 16 B. W. G. copper wire, and it gave most admirable results as far as conductivity was concerned. Its only defect was that the metal was so extremely pure that where the poles were shaken by the camels the wire elongated to such an extent, that in many places it had to be tightened up again. In some instances

interference by the camels was prevented by trenches being dug round the poles. These answered very well, but where the poles were exposed the wire dropped lower and lower and had to be tightened up again. With regard to the recorder instruments my experience has led me to an opposite conclusion to that of Colonel Home. I prefer the sounder. One of my people got deaf from the effects of quinine. I found the sounder instrument quicker in working, and good clerks can do quite as well or better with the sounder instrument than with the recorder. Where an instrument gets a little shaky from use you can get good results by sound where printing is out of the question. As I have already stated, the wire I used was No. 16 B. W. G. pure wire and consisted of about 96 per cent. of copper. I have tried No. 15 homogeneous iron wire, which gave good results when tested in England, but failed utterly in Abyssinia. I used it to the extent of about 20 miles of line, but I think it was over-galvanized; wherever the zinc was rigid it broke in making the ordinary German joint. Spans of 100 yards were difficult to make, whereas with a No. 16 copper wire I could carry out spans of 200 and 300 yards.

The Chairman: What was the difficulty with regard to long spans with the iron wire?

Major St. John, R.E.: It broke with its own weight; it was not up to sample; and as far as conductivity is concerned it was not equal to the copper wire. If I had had anything to do with the sending out of the wire for the Gold Coast, I think I should have suggested a covered wire to be carried through the forests.

Lieutenant-Colonel Home, R.E.: I thought of that myself, but the ants would eat through gutta percha and indiarubber.

Major St. John, R.E.: Did you try it?

Lieutenant-Colonel Home, R.E.: I did not try it. I know that an indiarubber pontoon was eaten through in a week, and in another week was entirely destroyed by the ants, and that prevented me from asking for insulated wire.

The Chairman: What description of covered wire have you had experience with?

Major St. John, R.E.: Hooper's. I took out 50 miles of it

as a purely military telegraph for temporary purposes, and it came back as good as when it was sent out, and was not deteriorated in any way.

Mr. Von Fisher Treuenfeld: I have erected military telegraphs, and I come to the same conclusion as Colonel Home as to the recorder being the most reliable instrument. I have used dial instruments, needle instruments, sounders, and recorders, and I have decidedly come to the conclusion that the recorder is the only reliable instrument for military field service. I quite agree with Colonel Home for the reasons he has explained. I regret to hear that an opinion prevailed that the compound wire could not be trusted, because it has not been fully tried. I know it has been very extensively used in the United States of America with the most satisfactory results, also in Brazil and even China, and it has everywhere given the greatest satisfaction. If the compound wire had been used in the Ashantee war, No. 11 gauge, instead of the wire weighing 200lbs. and more per mile, it would only have weighed 65lbs., which, in the matter of rapid transport, would have been of great consideration. In my opinion the compound wire is of all others the very wire for military telegraphs, on account of its lightness and great strength.

Mr. Matthieson: There is one point which requires explanation with reference to the ebonite insulators. Lieutenant Jekyll has not explained that they were spiked insulators for the purpose of being driven into trees, and I believe it was the driving them in with a hammer which loosened the cup from the stem. Perhaps Lieutenant Jekyll will be good enough to say how that was.

Lieutenant Jekyll, R.E.: A large proportion of the cups were loose when they were unpacked, and fell off as they were taken out of the cases.

Major Malcolm, R.E.: I attended the unpacking of some of the telegraph stores returned unused from the Gold Coast, and I found the ebonite cups had been fastened with some compound which had not been successful. There is one thing which Colonel Home should be reminded of with regard to the instrument employed, viz., that what may be suitable in a hundred cases may not be

applicable in some particular case; and I do not see because one man may be deaf we are to cast aside the sounder altogether. Hard cases make bad law. No doubt in a military campaign the recorder may be a desirable instrument to employ, but I would suggest that, before a message is sent on, it should be returned to the station from which it originated and there ascertained if it has been correctly transmitted. That would have the advantage that no mistakes would leave the office. The recorder is, of course, just the machine to discover who made the mistake after the damage has been done, but, if you return the message to the office from which it originated before it is finally transmitted, you avoid mistakes. I may state I tried to introduce that on one short line at Chatham last summer, and the only mistake made during the whole time was that when in one instance the message was not returned to the office from which it orignated, *beer* was ordered, when *beef* was wanted. There is no difficulty in the use of sounder instruments, and I believe it is a growing practice with the Post Office Telegraphs to adopt them. I may just mention, in illustration of the great value of the telegraph in the Ashantee Campaign, that directly after the capture of Coomassie a telegram was sent to the general from the Prah that the river was rising, which fetched the army back at full speed. The absence of the telegraph might have made it impossible for the troops to cross the river, and that one message therefore far more than repaid the whole expense of the telegraph.

Mr. W. H. Preece: I am authorised by Major Webber to say that whatever promptitude and despatch was shown in sending out the telegraph stores for the Ashantee Expedition was entirely due to Mr. Scudamore. He responded at once to the application made to him by the War Department, and the whole establishment was put under requisition. Orders were instantly sent out, and the wires and other materials were sent down to Plymouth as soon as possible. With regard to the question of sounder *versus* recorder instruments, my opinion has not in the least been shaken by what Colonel Home has said. I am an advocate for reading by sound. Sounders are being introduced into the Post Office Telegraph

system as fast as they can be. Their great merit is, that, apart from supplying an instrument of the simplest character, you introduce an instrument which of itself produces that accuracy which Colonel Home is so anxious to obtain. The reason of the sounder's accuracy is this: it depends upon the ear, and when once you get a man competent to read a despatch by ear you secure a man who can send a despatch with the same accuracy, for the hand follows the ear. It is said with regard to the recorder that you have something to show afterwards where the inaccuracy takes place. My answer to that is, with the sounder you have no error to record. It is not because a man is deaf, or that he has lost one sense, that he is prevented from reading or receiving a message by the sounder. It is on record that on the occasion of the death of one of the Presidents of the United States its announcement was read in different parts of the country by every single sense which we possess—generally by the ear, sometimes by the eye, in one case by the tongue, in another case by the touch, and in one instance a blind girl read it by smell (laughter.) She placed her nose above the instrument, which was Bain's chemical recorder, and thus deciphered the despatch.

Lieutenant Jekyll, R.E.: I have but one or two remarks to offer in reply to what has been said on this subject. In the first place I should say the insulators appeared to be defective in manufacture. The cups were loose upon the shanks when the insulators were unpacked, and they were not broken by the driving in of the spikes, because the spikes would not drive. With regard to compound wire, before it could be definitely adopted as the best material for military telegraphs it would be necessary to see whether it is useful for other purposes as well. There is frequent demand for wire for many military engineering purposes apart from the telegraph, and if the compound wire is not available for such it would be no improvement to introduce it in the place of ordinary iron wire.

The Chairman: I will not trouble you with any remarks of my own, although I might be able to add a little to the interesting discussion we have had. I will simply propose a vote of thanks to

Lieut. Jekyll for the interesting and valuable and useful paper he has read to us this evening.

The proposition was unanimously adopted.

The SECRETARY read the result of the ballot for President and Members of Council for the ensuing year as follows:—

President.

LATIMER CLARK, C.E.

Past-Presidents.

SIR WILLIAM THOMSON, F.R.S., LL.D.
FRANK IVES SCUDAMORE, C.B.
CHARLES WILLIAM SIEMENS, F.R.S., D.C.L., M.INST.C.E

Vice-Presidents.

PROFESSOR ABEL, F.R.S.
R. S. CULLEY, M.INST.C.E.
PROFESSOR G. C. FOSTER, F.R.S.
C. V. WALKER, F.R.S.

Members.

MAJOR J. U. BATEMAN-CHAMPAIN, R.E.
H. G. ERICHSEN.
EDWARD GRAVES.
MAJOR MALCOLM, R.E.
W. H. PREECE, M.Inst.C.E.
ROBERT SABINE, C.E.
CARL SIEMENS, M.Inst.C.E.
C. E. SPAGNOLETTI.
LIEUT.-COLONEL STOTHERD, R.E.
CROMWELL FLEETWOOD VARLEY, F.R.S., M.Inst.C.E.
EDWARD WILDMAN WHITEHOUSE.
PROFESSOR WILLIAMSON, F.R.S.

Associates.

THOMAS ANGELL.
JOHN BAILBY.
WALTER HANCOCK.

OFFICERS.

Auditors.

J. WAGSTAFF BLUNDELL.
FREDERICK C. DANVERS (India Office).

Hon. Treasurer.

MAJOR C. E. WEBBER, R.E.

Hon. Secretary.

MAJOR FRANK BOLTON.

Secretary.

GEO. E. PREECE.

Mr. Walter Hancock: I have much pleasure in proposing a resolution which I am quite sure will meet the cordial acceptance of all present and of every member of the Society of Telegraph Engineers. In considering the business of the last Session, our warmest thanks are due to the late President for the active part and warm interest he took in the proceedings of the Society. The name of Professor Sir William Thomson is a household word with every electrician, every scientist, and every telegraph engineer. A man who has accomplished three things of such importance as his laborious investigations into the subject of the speed of transmission of telegraph messages, that marvellously beautiful instrument his galvanometer, and that very wonderful yet simple instrument the quadrant electrometer—these things are sufficient to stamp him for all time as a man who has left his mark on electrical science and an indelible mark on telegraphy. I will simply move the resolution, "That the thanks of the Society be accorded to Professor Sir William Thomson for the manner in which he filled the chair during the past year, and for the assistance he has afforded the Society in furthering its proceedings."

Mr. Sivewright, M.A.: I have great pleasure in seconding, as an Associate, the vote of thanks to Sir W. Thomson proposed by Mr. Hancock. I can do little more than simply endorse every word he has said. We may consider ourselves exceedingly fortunate in having had Sir William for our President during the past year; for, whilst the internal success of a society like this must to a great extent depend upon the exertions of the members and the amount of energy which they throw into the work, our status is yet too young, and we must depend for success in a great measure upon the names of those who act with us by becoming our officers. In Sir W. Thomson we have a most distinguished scientist, who now stands in the foremost rank of telegraphy, and, looking at his year of office, he has been something more than the figure-head of the Society. In his Inaugural Address he pointed out what were the proper objects of the Society. He dwelt upon the usefulness which its establishment would be in the wedding of science with practical application, and pointed out what are now the best means for

securing this end. If he had done nothing more than this, we must have felt grateful to him. Yet, at what must have been inconvenience to himself, we had the pleasure of his presence at several meetings during the year; and his advice at the meetings of the Council was as highly appreciated as it was readily given. We wish Sir W. Thomson should know that in retiring from the office of President he carries with him not only the best wishes but the gratitude of us all; and we can only hope that future Presidents will emulate the bright example he has afforded.

The resolution was put by the Chairman and carried unanimously.

Colonel STOTHERD, R.E.: The members of this Society are no doubt aware that by the kindness of the President and Council of the Institution of Civil Engineers we have now had the use of this magnificent lecture theatre for the last three years. I think the conveniences thus afforded have contributed materially to the prosperity of our Society, besides having enhanced the pleasure with which we have listened to the papers and discussions which have taken place from time to time. I will not occupy your time further than to ask you to pass the resolution which I have now the pleasure to propose, viz., "That the thanks of the Society of Telegraph Engineers be accorded to the President and Council of the Institution of Civil Engineers for their continued liberality and kindness in affording the Society the free use of this magnificent hall and other apartments."

Major WEBBER, R.E. seconded the resolution, which was unanimously adopted, and the business of the general meeting terminated with a ballot for new members.

The following Candidates were balloted for and declared duly elected:—

AS FOREIGN MEMBERS:—

Don José Alicé . . .	National Telegraph Department, Argentine Republic
,, Camelo Almaestre .	do.
,, Francisco Ahuamada .	do.

Don Jose Aparicio . . .	National Telegraph Department, Argentine Republic
C. Bachelor	Newark, New Jersey, U.S.
T. A. Edison . . .	Newark, New Jersey, U.S.
Don F. de P. Garcia . .	National Telegraph Department, Argentine Republic
„ Luis Lagore . . .	do.
„ Juan Lopez . . .	do.
„ Pedro Lopez . .	do.
„ Emilio Mori . . .	do.
„ Leon Schopinski . .	do.
„ Jacinto Sosa . .	do.

As Associates :—

George Draper . . .	Eastern Telegraph Company
Lewis Wells . . .	do.
Thomas A. Bullock . .	do.
William Ash . . .	do.
Edward Bull . . .	do.
George B. Stacey . .	do.
William Tuck . . .	do.
Charles Stacey . . .	do.
Robert Portelli . . .	do.
Francis Harwood . . .	do.
Benjamin Smith . . .	do.
Douglas Gibbs . . .	do.
Charles J. Murphy . .	do.
W. P. Binney . . .	do.
A. Thomas . . .	Brazilian Submarine Telegraph Company
J. Packham . . .	Mitcham
C. M. Hibberd . . .	General Post Office
J. Bedborough . . .	Kurrachee
H. Quilley . . .	General Post Office
C. W. Lundy . . .	Leadenhall Street
E. H. Thompson . . .	Alipore, India

W. K. Walker . . .	Lahore, India
A. J. Faulding . . .	West Drayton
R. Batchelder . . .	Bolsover Street
R. O. Bourne . . .	Brisbane, Queensland
A. E. Newman . . .	Buenos Ayres
F. F. Talbot . . .	Rosario, Argentine Republic
Louis J. Daniel . . .	Melbourne
Henry Jenvey . . .	do.
David Mickle . . .	do.
Arthur Oatway . . .	General Post Office
Martin Roberts . . .	Hackney
Henry Stokes . . .	Alexander Square, S.W.
Leonard Wray . . .	Crosby House, Bishopsgate Street

As Students:—

Basil Gee . . .	Cambridge Street, W.
J. Kirkman . . .	Hampstead
G. H. Thompson . . .	Lee, Kent

The meeting then adjourned.

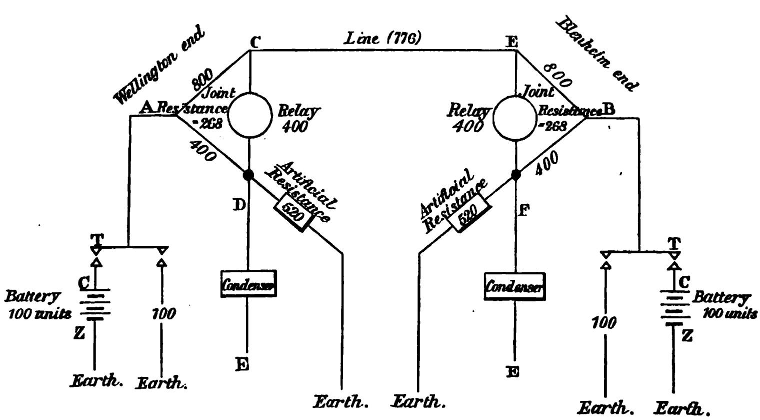
ORIGINAL COMMUNICATIONS.

DUPLEX TELEGRAPHY IN NEW ZEALAND.

By C. Lemon,
New Zealand Telegraphs.

Since June 18th, 1874, I have successfully employed the Duplex system on one of the wires in the Cook's Strait Cable. As the system I have here perfected differs somewhat from that in use in England, a description may be interesting to the members of this Society.

My attention had been drawn to the duplex system by an extract from a paper read by Mr. Culley before the Society, printed in the *Telegraphic Journal.* In this extract no minute details were given, so that it was necessary to work out the system from the principles laid down. Starting with the bridge system, after a number of experiments dating from the 15th April, I succeeded on the 25th of the same month in establishing a balance which has worked well under all kinds of weather. I found in some first experiments trouble from the "kick" as it is termed; this defect has now completely disappeared. My arrangement is as follows:



All figures Siemens units.

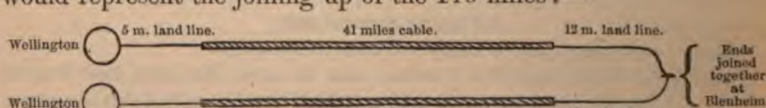
Battery:—10 cells, equal to nearly 100 units in resistance.

Line:—Wellington land line 5 miles, cable 41 miles, Blenheim land line 12 miles = 58 miles = 756 S.U. or 776 S.U. including the 2 galvanometers, 1 at either end, each of the value of 10 S.U.

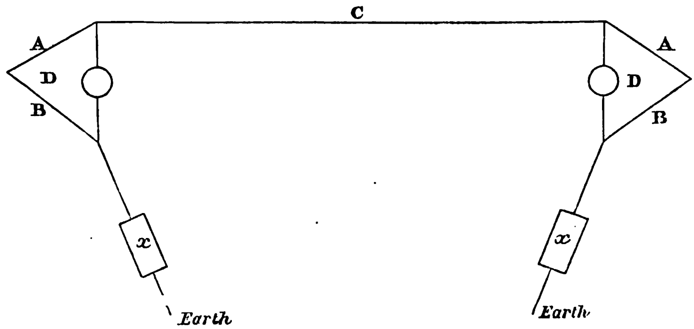
The joint resistance, 268 at either end, is composed of upper limb 800, lower limb 400, relay 400, back contact-key 100 (representing same value as battery, so that whether with key up or down the same, or nearly the same, resistance is in circuit). The point from D to earth, or from E to earth, had no resistance in when testing for joint resistance of relay, limbs, &c., and the test for joint resistance is taken at E or C, as the current starting from B would measure the resistance at C, and from there or at that point would divide, passing through the relay and through the upper limb (800). The same would occur with the current coming from A and arriving at E. This joint resistance at either end obtained, the resistance of the line is added to the joint resistance, making a total resistance of 1,044 units, and half this amount, 520 S.U., comprises the artificial resistance. It should properly be 522, but the resistance I had made only went as low as 10 units, and comprised in all 600 units, the last 100 being made up of coils of 50, 20, 10, 10, 10 units.

These proportions in the bridge (800 and 400) were not arrived at without some trouble, but the success attained amply repays the labour. Most of my experiments were conducted between 9 p.m. and 12 p.m. on consecutive nights. When once fairly started in the investigation I left no stone unturned to bring about the desired end.

From the arrangement shown in the plan I have laid down a formula by which I can work out the system for the future on any land-line or cable; and I have tried it on land-lines of 200, 86, 20, and 116 miles (containing cable and land-line) respectively, the last-mentioned mileage comprising 82 miles of cable and 34 miles of land-line. My first experiments were made on the Cook's Strait Cable, and it comprised two of the cable wires joined at Blenheim, so that I had both stations in my own room, and was thus able to see and to meet the difficulties as they arose. The figures below would represent the joining up of the 116 miles:—



My formula, and method of arriving at the same, is as follows:—



Let A be a resistance twice that of B; let C be the resistance of the line, and D the joint resistance of the relay, bridge, and back contact of key at either end. Then, as A is to B, so is C + D to X, or $X = \frac{(C + D) \times B}{A}$; or in figures if—

A = 800 = the upper limb of bridge,
B = 400 = the lower limb of bridge,
C = 776 = the measured resistance of the line including galvanometers,
D = 268 = the joint resistance of either set of apparatus as described;
then X = 522 = the amount of resistance to be inserted in rheostat.

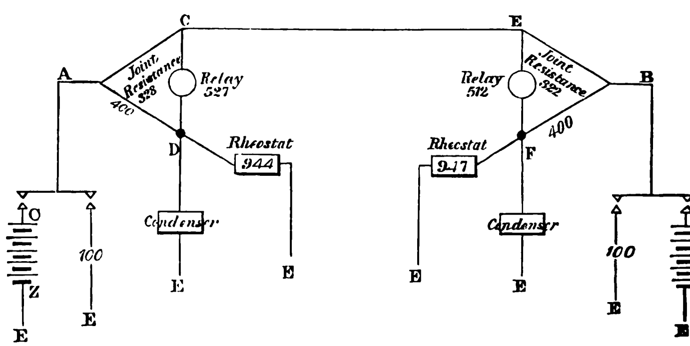
The relay, it will be observed, in plan is equal to one-half the upper limb of bridge, but I have had as high a resistance as 527 and 512 at either end. Of course the joint resistances at either end would be more than 268, with the same proportion in bridge. The relays which I am working with on the cable now I have had reduced to 400 each, so as to get a better proportion; I may say, however, that I found the higher resistance relays work just as well and give just as good results; and in working duplex on any of the land-lines I shall possibly not go to the trouble of rewinding the relays but use them as they are.

The condenser I use is equal in capacity to (perhaps) a little more than one microfarad, and one is placed on each relay as shown in the plan. The condensers are so arranged that I can increase the number if required, but up to this time I have found the number I use sufficient.

The one wire now carries on an average a message a minute. Of course on so short a line we can work at a very high speed—nearly 26 words per minute each way. On our first working duplex on the single cable, when joined up for business, we sent one way 106 words in three minutes, and received at the same time 87 words, making an average of 64 words per minute, or 32 each way. Every signal came out well and distinctly, without the slightest sign of a "kick."

After I had obtained the proper balance, and whilst the apparatus was being made necessary for the permanent working, I tried several experiments (all my experiments being with the double cable and according to the following diagram):—

Cable and Line 1566, as shown in Diagram 2.

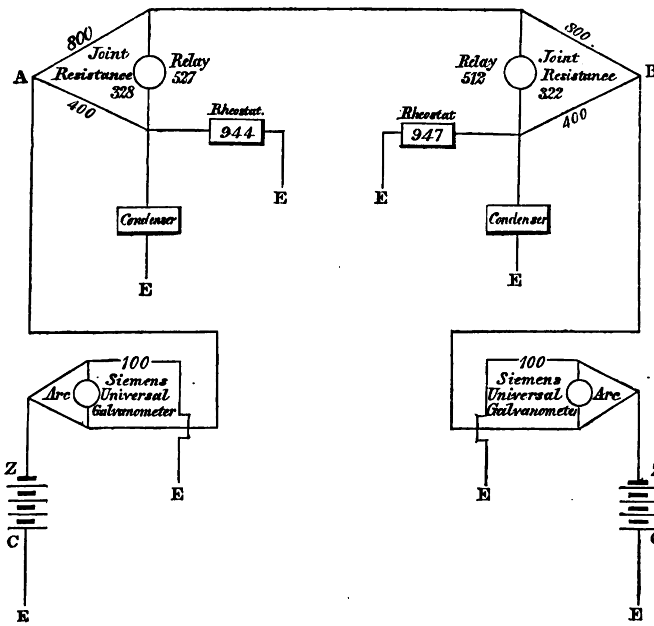


Battery 10 cells each end.—Copper to line both ends. All figures in Siemens units.

I found that after I had obtained the proportions in rheostat (as per diagram), and everything was working well, with capital signals at both ends, that I could insert 100 units on the line without apparently disturbing the balance or affecting the signals in the slightest degree. I was also rather curious as to what was the

actual resistance when both currents were on the line, and in accordance with the following diagram arrived at that result:—

Line 1566.



I tested with two Siemens universal galvanometers, using the line battery of A and B respectively, and with zinc to line—the above arrangement of galvanometers taking the place of keys. Of course, when both currents were on line, back contact of key would be cut out, and the resistance of the battery inserted in place of that resistance.

Both currents were put on simultaneously, and the vernier adjusted till the needle of either galvanometer was brought to zero. The reading of the arc at the A end—which would be the resistance from A to the distant end—gave 127·5 = 1,233 Siemens units, and that at B treated in like manner gave a reading of 128 = 1,264 units.

The galvanometer at the B end was more sensitive and the needle much lighter than that at A; consequently the test at B

(128 = 1,264) may be looked upon as correct; the other end may not be so nearly correct. It takes some time to get used to an instrument, and very likely the reading was half a division out, which, if so, would bring it up also to an arc of 128.

It would appear from the test that this resistance of the line does not enter into the calculation. For the sum of the resistance at A (that is, of the joint resistance 328 and the resistance of the rheostat 944) is a little more than what the test gave at A (1,233); whilst those at B (322 and 947 = 1,269) are a little more than what the test gave at B (1,264), so that it may be fairly concluded when both ends are sending the resistance of the line is practically nil. I may, however, be wrong in this assumption.

I also found on the land line of 20 miles that I could—after obtaining the resistance and adjusting the rheostat accordingly—insert a resistance of 300 units in the line (the line only measured 292·1 units), and still work the duplex as well as before.

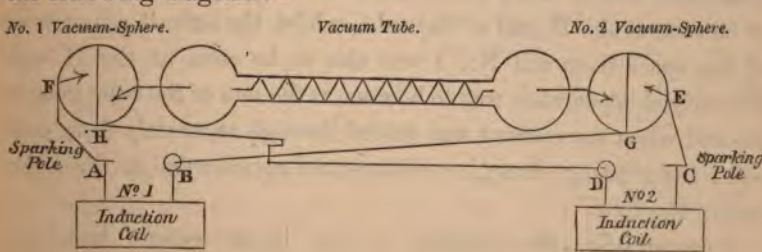
I may mention that I have tried with copper at one end of the line and zinc at the other, and have obtained similar results.

The system was subsequently tried on the 400 miles circuit, with a battery of 30 cells, and their corresponding resistance inserted at back contact of key. Adaptation has also been made to cases of constant current, and this is easily effected by simply reversing the position of the battery and back contact, so that when the key is in its normal position the current is on the line. Signals are made by putting the front contact to earth through the resistance inserted to balance the battery placed at the back contact. The battery must have its poles reversed; but the relay at the distant station is so placed as to receive a current opposite to that sent by the battery. Thus, if the zinc current be sent to line the relay (one of Siemens's polarised relays) will be placed as if to receive a copper current. The local circuit is closed only when the front contact of key is put to earth, for the zinc (continuous) current acts as a repellant and keeps the armature on the insulated stop. The relay is so adjusted that when the current is cut off the local circuit is closed. The joint resistance, which includes the value of the resistance of the battery, is first taken by transposing

the resistance that is placed in the front contact to the back contact, and disconnecting the battery for the time. This plan of continuous or constant current is much more simple than that described by Mr. Culley as perfected by an engineer at Madras.

The method given by Mr. Culley, in which an automatic key is employed in the duplex, is, I consider, open to the grave objection of loss of battery power. The diagram (facing page 400, fig. 141, of Mr. Culley's *Handbook of Practical Telegraphy*) shows that, at the instant of time when the current brings down the keeper on the magnet, there must be a loss of battery power; for before the spring is released or thrown off the earth connection there is evidently a split circuit, part going to the line, but the greater part going through x to earth. I have made an addition to Siemens's Morse key, which avoids the difficulty, and does away with the "local battery" circuit described in the figure above referred to.

I have given previously a diagram of a test taken from both ends of the line at once, the results of which seemed to me to prove that the resistance of the line was nil when both currents were on. Subsequently I have made another experiment with the aid of two induction coils (each coil giving 1½-inch *spark* in air), and I am inclined to think that the currents sent from either end do actually pass one another, or at any rate exchange impulses, and so record the opposite signal. It must be borne in mind that, if the currents *do not pass* but exchange impulses, they obey the same law as two bodies in motion moving with equal velocity and meeting each other. Any retrograde motion on the part of either is undoubtedly due to the forward motion of the opposite body. My experiments with the two induction coils were made as shown in the following diagram:—



The two spheres ($1\frac{1}{2}$ inch in diameter) had platinum wire points inside at a distance of about half an inch, and a platinum wire that passed through from side to side of each sphere as shown in diagram. The sparking poles A and C of the two induction coils were connected to F and E of the spheres and the receiving poles B and D of the coils to G and H. Thus the spark from No. 1 coil would pass from A to F, then through the No. 1 sphere across the vacuum tube on to G, and back to its own receiving pole B. Sparks from coil No. 2 would pass from C to E, then through No. 2 sphere, the vacuum tube on to H, and back to its own receiving pole D. By this means the phenomenon of two currents of electricity passing one another was rendered visible.

Further, I detached the two receiving poles from G and H and joined them together, so that the spark passing from No. 1, before it could get back to its own receiving pole, was forced to traverse the secondary wire of coil No. 2; and the spark from coil No. 2 had to do the same as to No. 1 before its spark could reach its own receiving pole. When the connections were made in this manner, now and again (not continuously) a spark would pass in either coil direct from its own sparking pole to its own receiving pole.

With one coil turned on, the light in the spiral tube was of some intensity, but with both coils the light was of double this intensity, thus giving double the quantity of light in the tube when both currents were on at the same time. This was most distinctly visible.

I also connected a Morse key between one pole of the battery and the make and break at each coil, and the flashes of flame could be distinctly seen discharging at either end of the vacuum tube. That from No. 2 had a cotton-wool or flossy appearance, and was to be seen at the H end of the tube, whilst the striped appearance of the spark from coil No. 1 was also to be seen at the H end. The striped appearance was to be seen at the end of the tube nearest the coil when the current was passed through separately from each coil; the glowing, flossy, or cotton-wool appearance at the other end of the tube.

Commercially, the adoption of the duplex system has been

attended, in the case of the Cook's Strait Cable, with the greatest success. The saving to the colony is estimated at about £30,000, or the cost of laying another cable, now rendered unnecessary. I have had the honour to receive a letter of thanks from the Government, and the House of Representatives has, in debate, expressed itself highly satisfied with my exertions.

In practice the method has continued to answer well. The balance in the bridge having been fixed before commencing work, any fault, such as signals too weak or too strong, may be compensated for by adjusting the relay. The best proof of the soundness of the system is the fact that, since the 18th of June to the present date, it has been working successfully in all kinds of weather—very wet, dry, frosty, and "muggy" atmospheres; the four kinds of weather we may look for at any time having thus been encountered.

Wellington, New Zealand, Aug. 30, 1874.

ABSTRACTS AND EXTRACTS.

ON THE DECAY AND PRESERVATION OF TIMBER.

By Monsieur Max Vantet.

I propose, in this article, dwelling on the destructive agencies which affect timber injected with sulphate of copper and embedded in railway ballast.

It is generally admitted that the preservative action of the metallic salt is due to its combination with the ligneous tissue, and above all with the nitrogenous matter of vegetable substances, the latter being thereby rendered insoluble and destructive to organic life. This explanation does not fully meet the case.

I have carefully studied the action of metallic salts, especially copper salts, on the nitrogenous substance of timber. The experiments I have made, extending over a long period, have demonstrated: 1st. That the cupric albuminous precipitate is not absolutely insoluble in water. 2ndly. That it is especially soluble in water charged with carbonic acid.

The nitrogenous matter contained in ordinary wood is partly soluble and partly insoluble. The soluble albuminous portion is fixed by the metallic salt, which likewise unites with the insoluble nitrogenous matter. Water, especially when containing carbonic acid, dissolves and carries off the salt.

These conclusions followed from my first experiments, but recent observations have proved that the reactions are not in all cases of such a simple nature. A result which is frequently observed is illustrated thus: A sleeper, of beech for example, originally saturated with sulphate of copper, after having been buried in railway ballast for a period of eight or ten years, on recovery is worthless, being found rotten at various points. The affected portions are very brown near the rails; the timber is not worm-eaten, but its character is chemically altered. If it does not contain sensible quantities of copper, it contains iron, occasionally in very large quantities, which has been obtained from the rail itself, or from the spikes.

This impregnation of iron, which has evidently travelled some distance

into the wood in a state of solution, has not prevented its decay. This result is quite contrary to pre-conceived ideas.

In researches of this nature, great care must be taken to eliminate portions of wood that have been in direct contact with the rail, or in such close proximity to it that scales of oxide might, by entering the cracks, vitiate the results of the experiment. This precaution taken, it will be found that the ligneous fibre in the neighbourhood of the rail has assumed a deep brown colour, that its strength is greatly diminished, and that it is easily crushed and pulverised. The density of this altered wood has singularly decreased; thus in the same beech sleeper the specific gravity of a sound portion is found to be 0·755 gr., whilst that of another which has been attacked is but 0·380 gr.

Wood, in the state referred to above, presents the following chemical characteristics: It contains nitrogenous matter; it is wholly dissolved in caustic potash; treated with water acidulated with nitric acid it gives up the lime it contains in addition to a large quantity of iron. This iron, which could only penetrate the wood in a state of solution, is now insoluble. Further, a solution of ferro-cyanide of potassium when applied to a freshly-cut surface of the wood causes no blue discolouration, notwithstanding the iron it contains.

During the action of nitric acid on the wood and the withdrawal of the iron, a considerable disengagement of carbonic acid is observed, as though an impure carbonate was being acted upon. The carbonic acid far exceeds the quantity I had observed in wood which had decayed whilst exposed to the air. In this which has decayed, when embedded in ballast, there is no comparison in the quantity of carbonic acid it contains and that which would result from the conversion of its ashes into carbonates by the slow combustion of its ligneous fibres. Need I add that new timber contains no carbonates and therefore no carbonic acid. A weight of 0·250 gr. of the above decayed wood (with a cubical measurement of 0·66 cc.) has evolved as much as 10·5 cc. of carbonic acid, being more than 12 cubic metres of this gas per cubic metre of wood. This timber, in the state described, contains a considerable quantity of ashes; when exposed to a red heat for a considerable period a residue of three per cent. of ashes remains; whereas beech in its normal state contains but one-half that quantity. When exposed to ebullition in acidulated water a portion of the wood is dissolved; further, when this solution is concentrated in a platinum capsule the residue before calcination becomes

blackened and carbonised. If the salts with which this wood is impregnated are extracted, the density is further diminished to 0·302 gr.

These remarks apply to the rotten portions near the rail or the spikes. Those portions of the sleepers somewhat removed from the rail do not exhibit this ferruginous character unless the ballast itself contains metallic oxides; but calcium carbonate is always abundantly found in the rotten portions. The copper is gradually and wholly replaced in the wood by calcium carbonate.

What changes have taken place? The calcium carbonate contained in the ballast, which is soluble in an excess of carbonic acid, gradually penetrates the wood and displaces the copper. To estimate the deterioration of the wood it is simply necessary to ascertain the quantity of carbonic acid or of carbonates which it contains. The tenacity of the ligneous fibres varies inversely as the quantity of carbonic acid which they contain. The copper retreats, if the expression is admissible, as the calcium carbonate advances. As long as the metallic salt maintains its original combination so long does the preservative action exist. The calcium carbonate is not the septic agent, but it eliminates the preservative agent from its combinations, and it separates the preservative matter from the matter to be preserved. The latter, therefore, if not restored to its original condition, is at least to one which facilitates the action of destructive agencies. This confirms and explains the fact already observed that sleepers are rapidly destroyed in chalky soils.

Sometimes the metal remains in the decayed wood as in the case already mentioned with reference to the deteriorated and highly ferruginous fibres in close proximity to the rail, but the oxide is, in such cases, mechanically and not chemically combined. Possibly the organic substance of the wood exercises a reducing action on the oxides, but most frequently when the timber has remained for a sufficient length of time embedded in the soil the cupric combination has ceased; the copper separated first from the albuminous substance is at last carried off by the carbonic acid from the tissue itself of the wood. Another solvent which, although less abundant, still aids the result, is the ammonia carbonate which is contained to some extent in rain water, or is furnished by the decay of the organic substances contained in the ballast.—*Association Scientifique.*

ELECTRIFICATION BY FRICTION AND FIGURES OF LICHTEMBERG.

By M. E. Douliot.

A plate of hardened caoutchouc or vulcanite, when dry, is electrified by the slightest disturbance,—by the friction of a sheet of paper, by that of the hair of a paint-brush. This can be readily shown, even to a large audience, by means of the following experiment: A plate of caoutchouc, being thoroughly dried, is passed over the flame of a spirit-lamp, with the object of removing all traces of electricity; then with a metallic point a piece of wood, or a paint-brush, a drawing or writing is prepared. All the marks remain invisible, as the instruments employed do not attack the surface of the caoutchouc, and care is taken not to scratch it. If now a mixture of sulphur and minium is scattered over the drawing the minium is attracted by the portions of the plate passed over by the drawing implement, and the sketch suddenly appears a bright red colour. The sketch is so clearly and distinctly defined that the finest lines are most clearly visible; and if a pen be used, and the slit is not perfectly closed, the two lines formed by the points, although in close proximity, are perfectly distinguishable. To obtain a similar result with a plate of glass, the surface must be rubbed energetically with a plug of wool, when, as might be anticipated, the sulphur is attracted, and the drawing appears yellow. A further experiment succeeds equally well, both with glass and caoutchouc. A plate of either substance having been electrified by rubbing it with a cat-skin, is written on with a metallic point. If now the sulphur and minium mixture is scattered over the surface, the characters appear in yellow on a red ground. In this experiment the lines are not so distinct and clear as in the former, as ramifications branch off and connect various parts of the figure or writing. The cat-skin may be replaced by the rubber of an electrical machine, as a means of exciting the plate.

NOTE ON A METHOD OF DISCOVERING THE POINT OF STOPPAGE OF A CARRIER IN PNEUMATIC TUBES.

By M. Charles Bontemps.

The French telegraphic administration transmits a portion of its telegrams in Paris by the well-known pneumatic system, in which carriers are driven by air-pressure through subterranean tubes. Hitherto, when a carrier has become stopped in the pipes, its locality has been ascertained by the following method. A reservoir of given capacity, containing a volume of air under a pressure H, is placed in connection with the faulty tube. On establishing a free communication between the two, the pressure is diminished by the mixture of the air in both in a certain ratio dependent on the capacity of the tube up to the faulty point. Let X be the capacity of the tube up to this point, and H the observed pressure in the reservoir after contact. The atmospheric pressure being taken at 0·76 m. we get the formula—

$$V H + X \times 0{\cdot}76\text{m.} = (V + X) H',$$

which gives the capacity of X and consequently the distance of the stoppage.

The accuracy of this method depends on the exactitude with which the volume V and the pressures H and H′ can be measured. In practice with the instruments ordinarily in use a reading approximating to the millimetre of mercury is difficult to obtain. Further, there are always other errors which vitiate the results. The experience obtained since 1866 in the removal of obstructions from pneumatic tubes with the aid of the manometer has proved that on an average three successive trenches have to be opened before the obstacle can be traced. The proposed new method admits of a much closer approximation in ascertaining the distance.

The principle of this new system consists of the method employed by M. Regnault in his researches on the speed of the propagation of sound-waves in tubes. At the open extremity of the faulty pipe an elastic membrane A B, fig. 1, is placed in such a manner that its vibrations may be registered, by means of electricity, on a revolving cylinder. A sound-wave is produced in the tube by the discharge of a pistol near the

membrane. This wave travels in the tube with a velocity of 330 metres per second, strikes against the obstacle, is reflected backwards, and,

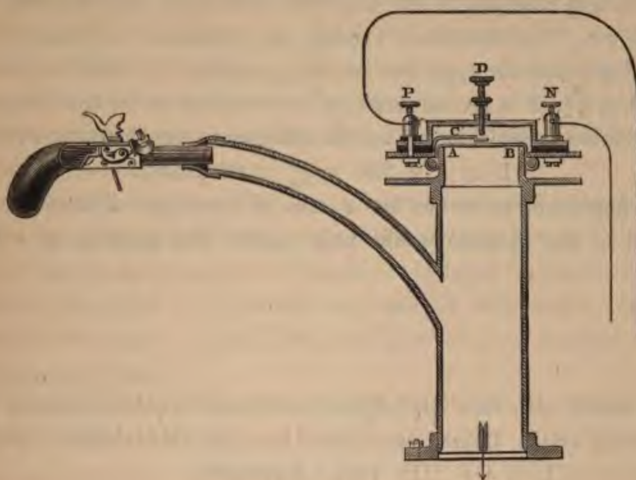


Fig. 1.

A B elastic membrane of india rubber. C a very thin metallic spring in electrical communication with a battery through the terminal P, but insulated from the apparatus D, a contact connected with a battery by the terminal N. The circuit is closed when the membrane presses the C against the stop D.

traversing the tube in the reverse direction, impinges on and inflates the membrane. This produces a mark on the cylinder. The wave is again

(Fig. 2.)

reflected backwards towards the obstacle, and on its return the membrane again vibrates, and so on. The recording cylinder carries three tracers or marking points, which are brought into play by electro-magnets. The first is in a circuit which is closed by the vibrations of the membrane, the second is in connection with a pendulum which causes it to record

seconds, whilst the third subdivides the seconds intervals by means of an electric make and brake.

A practical example of the employment of this apparatus may illustrate its use. An obstruction existing at a distance of 62 metres from the testing point, the apparatus on being applied is found to record an interval of $\frac{12}{33}$ of a second between two oscillations of the membrane. The following formula then gives the distance to the obstruction :—

$$D = \frac{1}{2} \times 330 \times \frac{12}{33} = 60 \text{ metres.}$$

This approximates within two metres of the actual distance, and the removal of the obstacle would only involve the opening of a single trench.

ABSTRACT OF REPORT FROM THE SUPERINTENDENT OF ELECTRIC TELEGRAPHS IN QUEENSLAND ON THE CONDITION OF HIS DEPARTMENT.

Electric Telegraph Department, Superintendent's Office,
Brisbane, 13th June, 1874.

SIR,—I have the honour to submit the following Report on the progress and condition of the Department under my supervision during the past year :—

EXTENSIONS COMPLETED.

The extensions completed since the date of my last Report are as follows :—

1. From Maryborough *viâ* Gin Gin to Teningering. This section consists of forty-eight miles of new line, and twenty-five miles of additional wire stretched on the existing poles between Maryborough and Gayndah.

The Teningering line is built of ironbark, bloodwood, box, and gum of good quality. It was completed and opened for business on the 1st of May last year.

The line has worked well from the commencement, and has already proved a great convenience to the inhabitants of Mount Perry, and those interested in the rich copper-mines of that district.

2. A line from Gin Gin to Gladstone, 104 miles in length, was completed on the 27th of October last. This section has been well built, of bloodwood, gum, and ironbark, and its completion opened up a second

through line between Brisbane and Rockhampton, much required for the large northern business.

3. A line within the railway fences from Toowoomba to Dalby, fifty-two miles in length, was completed on the 24th February in the present year. This line consists of three wires. Iron poles are erected on the plains in order to secure the lines from the effects of atmospheric electricity, unusually prevalent and destructive during the summer months on this portion of the Darling Downs.

4. A branch line from Gin Gin to Bundaberg, 30½ miles long, was completed and opened for business on the 7th of last March.

5. A line from Springsure to Tambo, 144½ miles in length, was completed on May 27th. The timber employed on this section consists of ironbark, bloodwood, cypress, pine, and gum of good quality, and will last for many years.

6. A second wire, 177½ miles in length, has also been erected on the existing poles between Brisbane and Maryborough. The line was completed and opened on 24th March.

All these lines have been substantially built in accordance with our usual specification.

New stations were opened during the year at Yandina, Mount Perry, Gin Gin, Bundaberg, Tiaro, Ipswich Railway, Walloon, Grandchester, Laidley, Gatton, Murphy's Creek, Highfields, and Toowoomba Railway.

We have 3,203 miles of line, 3,931 miles of wire, and seventy-four stations at present in operation, and 181 officers permanently employed by the Department.

The first line erected in Queensland (twenty-five miles in length), from Brisbane to Ipswich, was opened for public business on the 13th of April, 1861. The progress of the Department is shown in the following table:

Year.	Miles of Line.	Miles of Wire.	No. of Stations.	Total Number of Messages Transmitted.	Gross Receipts Cash.	"O.H.M.S." Business.	Gross Expenditure.	Population.
					£ s. d.	£ s. d.	£ s. d.	
1861	169½	169½	7	5,678	938 14 9	122 12 2	1,652 5 6	34,367
1873	3,059½	3,609½	73	156,268	20,759 1 3	6,386 12 0	27,776 8 5	146,690

This result is very satisfactory, and speaks well for the energy displayed in telegraphic enterprise by the inhabitants of this large but sparsely populated country.

Working of Lines.

The several lines in Queensland have, on the whole, worked well throughout the year: serious damage, however, was caused to the sections between Hawkwood and Camboon, and between Maryborough and Gayndah, by heavy storms and bush-fires in the month of November last.

Many miles of the line were also injured between Marlborough and Nebo by a cyclone in January this year; and, although every exertion was made to repair the line, communication was interrupted for several days.

Arrangements are in progress to form a school in connection with the Central Office, for the purpose of training efficient operators for service of the Department.

The railway telegraph between Brisbane and Toowoomba is now worked by Morse's recording instruments, under the supervision of this Department. The system works well, and will be extended to every railway station in the colony when the instruments ordered for that purpose arrive from England.

International Communication.

The South Australian Overland Line, between Adelaide and Port Darwin, has worked well throughout the year; the interruptions were less frequent than anticipated, and not more than on other lines of a similar length in the Australian colonies.

However, notwithstanding the efficient working of this line, the heavy cost of construction—on which £400,000 is said to have been expended, and £80,000 more required to complete the work—will prevent a reduction in the excessive charges for international messages until a competing line is established, when the present high tariff, namely, £9 16s. 6*d.* for a message of twenty words from Brisbane to London, will be considerably reduced.

I mentioned in my last annual report that negotiations were in progress for the purpose of establishing a second and independent line from Australia to Europe *viâ* Normanton and Singapore, and, before concluding, I would briefly refer to the origin and progress of this important under-

taking. In a letter addressed to the Honourable the Colonial Secretary, dated the 9th of December, 1872, I recommended that the proposed cable, in lieu of being taken from our terminal station at Carpentaria to Java, as at first proposed, should be carried to Singapore, in order to open up a through duplicate line between Australia and Europe, and, by avoiding the Netherlands Indian Government lines in Sumatra and Java, English operators would be employed throughout.

On the 3rd of February last year this proposal was submitted, by the Colonial Secretary of Queensland, for consideration of the Intercolonial Conference, then assembled in Sydney, when a resolution was passed in favour of a cable being laid between Normanton and Singapore without delay.

On the 10th of the same month the Queensland, New South Wales, and New Zealand delegates mutually agreed, subject to the approval of their respective Parliaments, to guarantee 5 per cent. per annum on the cost of constructing submarine lines between Normanton and Singapore, and from New South Wales to New Zealand.

Resolutions passed the Legislative Assembly and Legislative Council of Queensland, sanctioning the construction of these lines, in June last year, and the proposal was assented to in New Zealand by the passing of the Telegraph Cables Subsidy Agreement Ratification Act in the following month.

Although approved by the Executive Council, no action was taken by the Legislature of New South Wales until the 27th of May last, when resolutions in favour of the cables were passed by the Assembly; and the resolutions were confirmed by the Legislative Council of that colony on the 11th of June, 1874.

Now that the necessary authority is obtained to construct these important lines, steps will be taken to commence the work without delay.

I have, &c.,

W. J. Cracknell,

Superintendent Electric Telegraphs.

The Honourable the Postmaster-General.

ON THE NEW CONTACT THEORY OF THE GALVANIC CELL.

By J. A. Fleming, B.Sc., F.C.S.

The contest that has for so long been waged between the supporters of the two theories of the galvanic cell, the contact and the chemical, can hardly be said to have been brought even now to a decisive issue. For although the contact theory, as originally proposed by Volta, received a fatal blow when the law of conservation of energy became clearly understood, yet in its place a new contact theory has arisen, supported by novel and important experimental evidence, which has again been placed by recent writers on electrical science in formidable opposition to its old rival.

The old contact theory of Volta had its origin in an entire ignorance of the science of energy. It simply referred the current produced through the circuit of a pile to the effect of the metallic contacts, and it ignored the thermal and chemical changes which are also necessarily present; but it had to be finally abandoned when once it became clearly understood that the appearance of a current involved the disappearance of some other energy, actual or potential, as an invariable accompaniment. The new contact theory may be said to have had its source in the discovery of Sir W. Thomson, that there is undoubtedly a difference of potential produced when dissimilar metals are placed in contact—a fact not only abundantly proved by Thomson by direct experimental evidence, but, as he has pointed out, confirmed in a remarkable way by the phenomena of the Peltier effect, which, when interpreted by the dynamical theory of heat, furnish the most reliable measure of its amount. These facts, together with others presently to be referred to, have been made to furnish the key to a fresh explanation of the dynamics of the galvanic cell, which I have ventured to call the *new* contact theory, as opposed to the old or voltaic one.

It is not possible, however, to define in a few words the precise details of the new theory; they can only be arrived at by collecting together the statements as we find them laid down by their authors. The object of the present paper is to draw the attention of those interested in this question to the objections that may be raised against this new contact theory—objections based on facts, some old, and some which perhaps may prove new, but all of which alike seem to throw fresh difficulties in the

way of this theory, although capable of simple explanation by the old chemical hypothesis. It will be necessary then to review briefly the precise statements of this new contact theory, in order to show exactly what are the points against which objection may be taken. This will be best accomplished by collecting the statements of its principal supporters, and arranging together their explanations of the phenomena which arise—

(1) When dissimilar metals at the same temperature are placed in contact,

(2) When one insulated metal is placed in a liquid capable of acting chemically upon it,

(3) When two different metals are placed insulated and unconnected in one such liquid,

(4) When the two metals are joined across by a metallic arc, or when two or more cells are joined up in series.

1. That the contact of metals is always attended with the production of a difference of potential between them was for a long time denied by ardent supporters of the chemical theory. De la Rive endeavoured to show that the effects observed might be attributed to oxidation; but his experiments are not conclusive; and to Sir W. Thomson belongs the credit of having established the fact by experiment, irrespective of his theoretical deductions from the facts of thermo-electricity. He thus describes his decisive experiment:—

"A metal bar insulated so as to be movable about an axis perpendicular to the plane of a metal ring made up half of copper and half of zinc, the two halves being soldered together, turns from the zinc towards the copper when positively electrified, and from the copper towards the zinc when negatively electrified." * The difference of potential he finds to be about ·6 or ·7 of that of a Daniell's cell when the metals are perfectly clean; but by oxidation of the copper it may be made equal to or even greater than that of a Daniell's cell. He has also shown that, if zinc and copper cylinders be connected by a wire, the electrometer detects a difference between the potentials of the air in the interior; and, lastly, that, if copper filings be allowed to fall from a copper funnel in contact with a vertical zinc cylinder, they convey a negative charge to a receiver placed below. Sir W. Thomson concludes that there is sufficient evidence to show that zinc and copper attract one another chemically at any distance if connected by a fine wire, and that, as Professor Tait

* Reprint of papers on Electrostatics and Magnetism, p. 316, sec. 400.

remarks, "when any two bodies of different kinds are brought into contact, there is a certain amount of exhaustion of the potential energy of chemical affinity between them, and that the equivalent of this is, partly at least, developed in the new potential form of a separation of the so-called electric fluids, one of the bodies receiving a positive, the other a negative, change, the quantity depending on the nature and form of the bodies."*

This is equivalent to saying that at the surface of contact there is perpetually a force tending to separate the two electricities in a direction perpendicular to that surface, while at all points ever so little within it there is no such force.

Professor Maxwell reiterates essentially the same facts. He gives Thomson's proof that the electromotive contact-force at a junction of two metals is represented by P J, where P = the co-efficient of the Peltier effect, or the heat absorbed at the junction due to the passage of a unit of current for a unit of time; and J is Joule's equivalent. He remarks that the electromotive force, as determined by this method experimentally, does not account for the whole electromotive force of a simple couple. This latter is in general far greater than that given by the Peltier effect for the same pair of metals. "Hence the greater part of Volta's force must be sought for, not at the junctions of the two metals, but at one or both of the surfaces which separate the metals from the air or other medium which forms the third element of the circuit."†

Professor Jenkin, referring to these experiments of Thomson, adds, that "In cases where no known chemical action occurs, as where zinc and copper touch each other, and yet difference of potential is produced, since this involves a redistribution of electricity, a small but definite consumption of energy must then occur; the source of this power cannot yet be said to be known."‡

2. It seems to be universally admitted that, when an insulated metal is placed in a liquid capable of acting chemically upon it, a difference of potential is produced between the metal and the liquid, a sudden rise in potential taking place in passing from the metal-surface to the liquid in contact with it, or that the metal becomes negatively and the liquid

* Thermodynamics, p. 62, sec. 107.

† Treatise on Electricity and Magnetism, vol. i. p. 302.

‡ Electricity and Magnetism, p. 55.

positively electrified, metals differing in the degree of electrification they can produce with any one electrolyte.

3. But, if we ask what are the conditions when two different metals are so immersed, we find the most contradictory statements given. Sir W. Thomson expresses his opinion thus in 1862: "For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is very near Volta's. I now think it quite certain that two metals dipped in one electrolytic liquid will (when polarization is done away with) reduce two dry pieces of the same metals when connected each to each by metallic arcs to the same potential,"* which seems equivalent to saying that there is no difference of potential produced other than that due to dissimilar contact. Thus also Professor Tait: "By interposing between two metals which have been electrified by contact a compound liquid or electrolyte, these metals are at once reduced to the same potential — a result which could not have been obtained by connecting them by any metallic conductor. By the passage of the electricity a portion of the electrolyte is decomposed, and the potential energy thus developed is equal to that possessed by the electricity while separated in the metals."†

Professor Jenkin advocates essentially the same views: "When two dissimilar metals are plunged side by side into a liquid such as water or dilute sulphuric acid, they do not exhibit *any* sign of electrification; the three materials remain at one potential, or nearly so. If while the two dissimilar metals are in the liquid they are joined by metallic contact to terminal pieces of one and the same metal, these terminal pieces will be brought to the same difference of potentials as that which would be produced by direct contact between the dissimilar metals."‡ This amounts simply to saying that, as long as no wires are attached to the plates of a single cell, there is no difference of potential; but, that when wires are joined on, the observed difference of potential is due to the contact of the *wire* with that metal plate to which it is dissimilar.

Again: "When a single metal is placed in contact with an electrolyte, a definite difference of potentials is produced between them; zinc in water becomes negative, copper in water becomes negative, but less so than zinc. If, however, the two metals are plunged *together* into water,

* Electrostatics, p. 317, sec. 400.

† Thermodynamics, p. 66, sec. 116.

‡ Electricity and Magnetism, p. 22.

the copper, zinc, and water forming a galvanic cell, all remain at one potential, and no charge of electricity is observed on any part of the system." "If a piece of copper be now joined to the zinc, it (the copper) will become negative, and the other copper plate positive, the difference of potentials being that due to the direct contact between the zinc and piece of copper *only*, the water having the effect of simply conducting the charge from the zinc to the copper plate and maintaining them at one potential."*

The foundation for these statements is found apparently in an experiment due to Sir W. Thomson. He finds that if half-discs of zinc and copper be arranged under a movable metallic needle maintained at a high positive potential, if they are connected by a wire or by contact, the needle moves in such a way as to show that the copper is negative and the zinc positive; while, if they are separated by a slight interval and connected by a drop of water, *no* difference of potential is observed. Professor Jenkin also lays great stress on the fact that, whereas copper in contact with zinc becomes negative, in a single cell with wires attached it is the wire attached to the zinc that shows a negative potential. This he holds to be conclusive that the junction of the wire with the zinc plate is the real seat of the electrical separation; although he admits that there may be a slight difference due to the liquid, and that different liquids may augment or decrease this difference.

In another place he says:—"If the voltaic theory of the cell were absolutely correct, the electromotive force of the cell would depend wholly on the plates in the electrolyte, and not at all on the solution employed to connect them."† But it has been found that the potential series of the metals is slightly changed by the solution employed to join the plates; in order to account for this fact it is necessary to treat the voltaic theory as incomplete. He adds, however, that the potential series of the metals for water, dilute acids, and ammonic chloride do not differ so much as to invalidate the theory, although the series for alkaline sulphides is quite different and anomalous.‡

* Electricity and Magnetism, p. 44.

† Electricity and Magnetism, p. 215.

‡ It may be remarked in passing, that this identity of the potential series for different acids may perhaps arise from a different cause, and not be altogether such a proof of the contact theory as Professor Jenkin concludes it is. Andrews has shown that when *one* metal combines chemically with *different* acids the amount of heat liberated is the same, or nearly so. Hence, if the metals be arranged in the order of

4. When the two plates in one electrolyte are joined by a wire, or when simple cells are joined up in series and the circuit closed by a wire, we find it stated that there is a constant separation of the electricities at the point of contact of different metals and a constant recombination, attended with decomposition, through the electrolyte. "Perhaps it is strictly accurate to say that the difference of potential is produced by the contact, and that the current which is maintained by it is produced by chemical action."* And, lastly, that in a series of cells the electro-motive force is due to the sum of the differences of potential produced by all the contacts.

The above quotations may be taken as affording the plainest notion of the new contact theory; and it will be seen that its fundamental propositions are briefly these :—

I. The two plates of different metals in one liquid are at the same potential when insulated and separated ; *i.e.* there is *no* difference of potential due to chemical affinity.

II. In a cell-series the gradual rise in potential, or the electro-motive force, is due only to dissimilar metallic contacts.

III. The chemical action in the battery is the result rather than the cause of the difference of potential, and is looked upon as an accompanying action rather than as the actual creator of the current, it having little or no share in the production of the difference of potential between the terminals.

These are, I venture to think, points not to be admitted as proved without further inquiry, and against which, as I shall hope to show, some grave if not insuperable objections may be urged, founded on other experimental evidence.

The first question to be settled is, then, whether in a series of cells the *whole* of the difference of potential between the terminals is due to the contacts, as above stated, or whether *any* portion is due to the tendency

their heat-producing power when combined with the same acid, that order will remain the same for most other acids. But the order is quite different when the metals are combined with sulphur or oxygen. It is true that this order is not the electro-chemical one; but various causes may interfere to disturb it. At any rate it is sufficient to show that this fact of the partial identity of the potential series for different acids cannot by any means be claimed as conclusive of the contact theory. Moreover, although the *order* may be the same for the different liquids, we do not know that the *coefficients* are the same for each metal in every dilute acid.

* Electricity and Magnetism, pp. 53-55.

towards chemical combination existing between the metals and the electrolytes; and, as a consequence, whether in a single cell the plates are at the same potential or at different potentials, owing to the difference of chemical action upon them. Now I think this point will be sufficiently proved if we can establish by experiment (i.) that a battery of cells can be constructed without any dissimilar metallic contacts and with terminal plates of the same metal, and which shall yet exhibit difference of potential and continuous current; for, if this is possible, it must follow that chemical affinity *alone* is capable of *creating* electromotive force as well as of maintaining a current, and that, in an ordinary cell-series, *some* part at least of the electromotive force is due to this cause, whilst the remainder is the result of the metallic contacts that may exist; or (ii.) if we can establish directly that the two plates in one cell are not at the same potential, as stated by more than one authority.

With regard to the first point it will be remembered that an old experiment of Faraday's proved that a current can be maintained and decomposition effected by a single cell where there is no dissimilar contact. It is not easy to see how this experiment can be explained by any form of contact theory; indeed it appears unanswerable. But, in order to leave no point unsettled by experiment, it seemed desirable to try and arrange a series of cells in which all dissimilar contact was absent, so that the difference of potential due to chemical action might be separated from that due to the contacts and rendered visible by the electroscope.

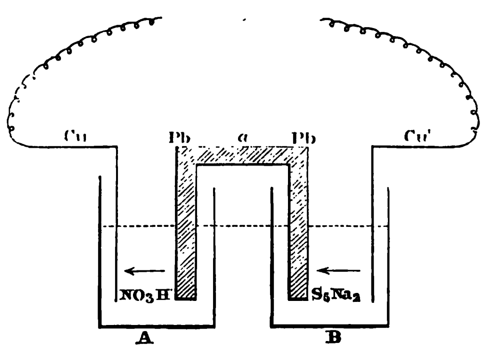


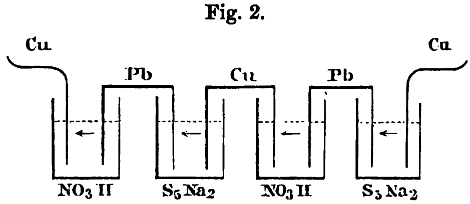
Fig. 1.

It is obvious that we can make no attempt to do this unless we can in some way or other obtain a battery with terminals of the same metals; for otherwise the very junctions with the electroscope introduce what we

want to eliminate, viz., dissimilar metallic contact. But the following is a method by which this can be accomplished. If plates of lead and copper be placed in nitric acid the lead is positive to the copper, since it is most acted upon; but, if lead and copper be placed in solutions of alkaline persulphides, then the copper is most readily acted upon and is positive to the lead; that is, the positions are reversed.

Now, if we place in a cell A dilute nitric acid and a copper and a lead plate, we cannot join up another cell of the same sort in series without introducing contact. But if, instead of using a cell containing acid, we place next A a cell, B, containing sodic pentasulphide, and bend over the lead plate of A to dip into the liquid in B, and place in B also a copper plate, we shall then have two cells joined up in series without dissimilar contact and with similar metals for terminals; and yet the action of the liquids on the metals is such that in A the lead is positive to the copper Cu, and in B the copper Cu′ is positive to the lead. Hence there is a regular rise in potential in passing through the two cells; and on joining Cu Cu′ by a copper wire a current flows through both cells in the same direction, and the general effect is to urge round a current in the direction shown by the arrows. It is obvious that we need not limit ourselves to two cells. By forming a pile of alternate cells filled with acid and alkaline persulphide, connected by bent copper and lead plates alternately (fig. 2), we shall be able to accumulate difference of potential to any

Fig. 2.



extent; and, if the number of acid and alkaline cells be equal, we shall always end with a plate similar to that with which we began. Such a battery will exhibit a difference of potential between its two terminals when the circuit is opened, and will give a current when it is closed. In it we have nothing but chemical action to rely upon both for creating electromotive force and for maintaining the current. We have no dissimilar contacts; and, as the terminal plates are similar, we can effect the junctions with the electroscope without introducing an unbalanced dissimilar contact. I have constructed such a battery of 60 cells; and by

the kindness of Professor Guthrie, to whom my thanks are due, I have been permitted to compare its potential with that of a Daniell's cell, by means of a quadrant electrometer belonging to his laboratory. By this means it is at once seen that the difference of potential increases proportionally to the number of cells, the electromotive force of four cells being about equal to that of one Daniell. Joined up with a galvanometer it indicates a current, which, however, rapidly falls off in strength, owing to the formation of an insoluble cupric sulphide upon the copper plates. Joined up in opposition to a single Daniell's cell, with a galvanometer included in the circuit, I find that it requires from four to five cells to balance the force of the Daniell at first immersion; but after leaving it to work on short circuit for 2½ hours its electromotive force had fallen off 50 per cent.; it then required about 8 cells to bring the needle to zero. This gives for the electromotive force of two cells at first about ·5 of a volt; or the whole sixty cells are equal nearly to 15 Daniell's cells. It readily effects the decomposition of many electrolytes, and exhibits therefore every property of an ordinary cell-series. Above all, it will be noticed, that since there is a regular rise in potential in passing from cell to cell, and as all parts of each plate must be at the same potential, that rise can only take place at the surfaces where the active metals are in contact with the electrolyte (that is, at the seat of the chemical action), and that therefore two metals in one electrolyte cannot be at exactly the same potential. But I find that more direct evidence still of this fact is to be found in an experiment of Faraday's, which seems to have escaped the notice of the contact theorists.

In his "Experimental Researches" he gives the following fact. "I took a voltaic apparatus, consisting of a single pair of large plates, namely a cylinder of amalgamated zinc and a double cylinder of copper. These were put into a jar containing dilute sulphuric acid, and could at pleasure be placed in metallic communication by a copper wire connecting the two plates. Being thus arranged, there was no chemical action whilst the plates were not connected; on making the contact a spark was obtained. In this case it is evident that the first spark must have occurred before metallic contact was made, for it passed through an interval of air; and also that it must have tended to pass before the electrolytic action began, for the latter could not take place until the current passed, and the current could not pass before the spark appeared." "Hence," he says, "I think there is sufficient proof that the zinc

and water were in a state of powerful tension previous to the actual contact."* It is difficult to reconcile this with the experiment of the half-discs and drop of water made by Sir W. Thomson. But, at any rate, a consideration of the whole of the facts would seem to point out that the only safe conclusion is, that in any series of cells of any sort the electromotive force is a complex effect, being due to the algebraical sum of all the differences of potential due to dissimilar contacts *plus* the algebraical sum of the differences of potential due to the chemical affinities of the metals and electroytes, *minus* any opposin gforce due to polarisation, &c.; and that, so far from being the exclusive cause, the contacts can only be said strictly to have a share in producing the difference of potentials between the extremities of a battery.† And, lastly, we may with advantage compare the statements of the contact theory with certain other well-ascertained facts. Such statements, for instance, as these: — "If we close the circuit by connecting the metals by a wire, we then have constant separation of electricities at the point of contact of different metals, and constant recombination attended with decomposition through the electrolyte."‡ "The electricities separated at the metallic junctions recombine through the water," "whilst the current flows the water is decomposed," §—which seem based on the assumption that the principal seat of the electrical actions is *not* to be looked for at the seat of the chemical actions. But, now, how does this fit in with those cases of electro-chemical inversions noticed by De la Rive, where the direction of the current in a cell is *reversed* by simply diluting the electrolyte? Thus zinc is negative to tin in strong nitric acid, and mercury negative to lead; but in weak nitric acid the positions are reversed. Hence, if couples be formed of these metals in strong nitric acid, and the acid be gradually diluted, the current first ceases, and then is reversed in direction.

Here, without altering the metallic junctions, we can at pleasure alter the direction of the current, and therefore also the direction of the fall in potential, since the current must flow from high to low potential. This seems conclusive that the chemical electromotive force must be even greater than the contact electromotive force. This reversal of the current,

* Experimental Researches in Electricity, series viii. par. 956.

† Amounting in a Daniell's cell perhaps to 60 or 70 per cent. of the whole electromotive force.

‡ Tait, "Thermodynamics," sec. 116.

§ Jenkin, "Electricity and Magnetism," p. 54.

by changing the seat of the chemical activity, may be shown in another way, depending on the application of a very old principle. If plates of copper and clean iron be connected by copper wires with a galvanometer, and the iron rendered passive by immersion for a moment in strong nitric acid, then if these plates are plunged into dilute nitric acid the galvanometer indicates a strong current going through the cell from the copper to the iron. If they be removed for an instant, and the iron plate touched, on again immersing the current is found to be reversed. Or we may again change the conditions, and notice that it is not sufficient to have merely two different metals and an electrolyte to form a cell. If plates of pure gold and platinum be placed in nitric acid, the most delicate galvanometer detects no current, and the same for many other pairs of metals and electrolytes.

Here we have contact of different metals producing its difference of potential; yet no current flows round "decomposing the electrolyte," as, according to the contact theory, it should do; but the instant we give play to chemical combination the ordinary results ensue. If the extremities of the copper wires from a galvanometer be attached to iron plates, and these plunged into separate cups of dilute nitric acid, on making connection between the two cups by a bent iron plate dipping into each no current is detected. On making one limb of the connecting-plate passive and re-immersing, a strong current is visible; and we find that we have the direction of the current completely under command by making any of the four plates more or less acted on than the other three.

If these experiments are to have any importance attached to them, it can scarcely be doubted that they land us in conclusions similar to the others, namely, that we must look for the principal source of the electrical disturbance at that place where the greatest chemical activity is being brought into play; and that, whereas contact of metals is in itself productive of definite electrical separation, there is in the battery another cause assisting in the production of difference of electrical potential between the terminals, viz., the potential chemical combination between the metals and electrolytes existing when the circuit is open—the energy of the current produced when the circuit is closed being, of course, the equivalent of this potential energy, which disappears.—*Proceedings of the Physical Society of London.*

ON THE GENERAL THEORY OF DUPLEX TELEGRAPHY.

By LOUIS SCHWENDLER.

INTRODUCTION.

The name of "Duplex Telegraphy" has been given to that mode of electric telegraphy which admits of the simultaneous transmission in opposite directions of signals between two stations through a single wire. That this name is far from happily chosen is evident; but, as it is current and has already gained a recognised footing, it is not considered advisable to endeavour to replace it now by a more rational one, and it will therefore be adhered to throughout this paper.*

In the following investigation I shall endeavour to develope the mathematical theory of "duplex telegraphy" in its most general form, with the object of determining not only the best arrangement for any particular method, but also the relative values of different methods.

It is manifest that, having from general considerations decided on the best method, and further determined the best arrangement for this method, the remaining difficulties, due to the nature of the problem itself, will be exhibited in a clearer light, and the means of overcoming them may then be more easily discerned.

It is believed, however, that the sequel will show, that if the best method be adopted, and for this method the best arrangement be selected, to suit the particular line on which the method is to be employed, the difficulties that stand in the way of duplex telegraphy will hardly be greater than those which are encountered every day in ordinary single telegraphy.

IMPERFECT HISTORICAL SKETCH.

Having access to but scanty records in this country, I am not in a position to give an exhaustive history of this most important invention, and consequently the following sketch is necessarily incomplete, and must be taken as merely introductory, it being relegated to those better situated in this respect than myself to clear up the doubtful points of priority, and produce, what is much required, a complete history.

* The German language possesses a peculiarly suitable word in *Gegensprechen*, and the idea is fully rendered by *Gleichzeitiges Gegensprechen.*

The idea of sending signals in opposite directions simultaneously through a single wire is by no means a new one. As early as 1849, Messrs. Siemens and Halske of Berlin took out a patent in England * for the simultaneous transmission of a plurality of messages by a suitable combination of wires, and, although this patent does not refer directly to duplex telegraphy as it was subsequently understood, it must notwithstanding be regarded as a forerunner of it. In point of fact Dr. Werner Siemens's idea represents the general problem, of which duplex telegraphy is only a particular case.

In 1854 Dr. Gintl of Vienna tried his "compensation" method of "duplex" working between that capital and Prague,† and on the 30th November of the same year read a paper before the Kaiserlich Königliche Academy of Science of Vienna ‡ on the practical solution of the same problem by employing a Bain's electro-chemical telegraph apparatus instead of a Morse's receiving instrument.

In the summer of 1854, after Dr. Gintl's experiments between Vienna and Prague had brought the subject prominently to notice, Messrs. Siemens and Halske of Berlin, and Herr Frischen independently, invented the "differential" method.

In January 1855, Edlund § made experiments on the line between Stockholm and Gothenburg. He employed a "differential" method, which he had invented in 1848 for the purpose of measuring accurately Faraday's "extra-currents."

In papers read at Paris on the 16th July and 6th August, 1855,‖ before the Academy of Science by M. Zantedeschi, he claims the honour of having first suggested the idea of duplex telegraphy, for as early as 1829 he had proved the possibility of the simultaneous transmission of currents in opposite directions through a single conductor. Having never seen his original communication of 1829 it is impossible for me to say how far these early ideas of Zantedeschi bear on the problem; but it is certain that both he and Dr. Gintl took a great deal of trouble to prove an erroneous theory, viz., that two distinct electrical currents can pass simultaneously in opposite directions through the same conductor

* 23rd October, 1849. The actual wording of the English patent is unknown to me.

† Polyt. Central bl. 1853, p. 1475.

‡ *Wien* Akad. Sitzungsber, xiv.

§ *Pogg.* Ann. 1856, vol. xcviii. page 634.

‖ *Pogg.* Ann. 1856, vol. xcviii. page 123.

without in any way interfering with each other. Such a supposition is in direct opposition to the electrical laws which were already known in 1829,* and besides is in no way required in order to explain the simple phenomenon of duplex telegraphy.†

None of the above methods, however, came to have extended, or indeed any, practical application. They appear to have been attempted doubtingly and without confidence, and, although the trials are generally reported to have been successful, yet the methods were rejected as impracticable, and came to be regarded as merely of scientific interest.‡

Only recently, after a torpid existence of almost twenty years, has duplex telegraphy been revived, and come to be the leading topic in telegraphy, securing, after such a lapse of time, the amount of public interest it rightly deserves.

To Mr. Stearns, an American telegraph engineer, is due the honour of having appreciated the real value of duplex telegraphy, and of having (by giving the system, modified by improvements of his own, an extended application on the lines of the United States) proved its thorough practicability.

Inquiry into the Causes which have delayed the Introduction of the System.

When Steinheil in 1837 announced his discovery of the feasibility of employing the earth to complete the electric circuit instead of a return wire, telegraph engineers immediately recognised its immense mercantile value, and did not delay to verify his results.

Now, in the career of telegraphy, the invention of duplex working ranks second only in importance to Steinheil's discovery. The utilization of the earth reduced by one-half the number of wires required to carry a given traffic: duplex telegraphy again almost halves this number. In the face of this fact it is not easy to understand why the one idea received immediate and universal application, while the other, of only about ten years' more recent date, has met until now with universal

* Ohm published his classical work "Die galvanische Kette mathematisch bearbeitet" in the year 1828.

† Dr. Werner Siemens, Pogg. Ann. vol. xcviii. page 123.

‡ For the *light in which* duplex telegraphy was regarded up till quite lately, see *Schellen, Dub, Sabine, Blavier, Kuhn*, &c.

neglect; but on closer examination it will be found that there have been perfectly comprehensible, although not all rational, influences at work.

An inquiry into the circumstances, therefore, that have caused the discovery of a system, the introduction of which must mark the second great era in telegraphy, to lie fallow for nearly twenty years is of the utmost interest, and cannot fail to be instructive with regard to the prospects of future progress.

From an examination of the methods originally proposed for duplex working, it will be found that they do not in any way essentially differ from those which may now come into actual use. The causes, therefore, which have prevented the introduction of the system must be sought for external to the methods.

The first of these, we find, is that the invention was in advance of the requirements of the age. Telegraph lines had already been constructed, which were quite capable of carrying the given traffic, and even more. Further, any increase in traffic could be met by an increase in the number of wires on the existing telegraph posts, instead of by resorting to a system which had a complex appearance, and after all might not answer.

However, although the above considerations explain the course of events in certain limited instances, and up to a certain time, they do nothing towards justifying the costly expedients that have been generally adopted until recently in preference to introducing duplex telegraphy; for instance, the reconstruction and multiplying of long overland lines, and especially the laying of a second submarine cable when the traffic became too great for one.

It is true that the successful application of any duplex method requires lines of a more constant electrical condition, receiving-instruments of a larger range,* and telegraph operators of a somewhat better professional education; but, surely, these three conditions have not all *at once* become fulfilled (since 1872) so as to make duplex telegraphy possible only just now. No; the causes which have delayed its introduction so long have been of a much less technical and more irrational nature.

* By the "range" of a telegraph instrument I understand the ratío of the largest to the smallest force by which the instrument in question can be worked without *requiring* a fresh mechanical adjustment. For instance, Siemens's beautiful relays *can be easily* adjusted to a range of 20, *i. e.* they can be made to work with one cell *through* an external resistance equal to their own resistance, and with 10 cells through no external resistance, *without* giving the tongue a fresh adjustment.

The mere fact of the duplex methods appearing complex prevented telegraph administrations from thinking seriously of introducing them. The ingenious methods were never tried with that zeal and perseverance which is necessary to carry a new invention successfully through. They were indiscriminately rejected after a few trials made without method or consideration, and the real conditions of success or failure were never examined or pointed out. Thus, naturally, a prejudice was created against duplex telegraphy, and it was fostered by a host of school literature up to the latest time, as pointed out before. Further, not a single physicist or electrician investigated the question with a view to ascertaining what quantitative effect the variable condition of lines has on duplex working as compared with single working.

If such an investigation had been made it would have been found that the technical obstructions in the way were by no means so formidable as had been represented, and that the electrical condition of the lines, as well as the perfection of the instruments and the professional education of the staff, would have fully admitted of the successful introduction of duplex telegraphy at least ten, if not twenty, years ago.

It is true, indeed, that the suggestion of using condensers for balancing the charge and discharge of a line has only been made very lately, being one of Stearns's happy ideas; but this should have been no reason against introducing the system on short and overworked lines, where the charge and discharge are imperceptible. If only one telegraph administration had shown the perfect practicability of the system on a short line, the cloud of prejudice would have been dissipated, and suggestions for overcoming the charge and discharge on long overland lines and submarine cables would have been readily enough given, and thereby large capitals saved.

To sum up, therefore, we have the following causes which acted persistenty against the introduction of duplex telegraphy:—

Firstly, the invention was in advance of the age.

Secondly, the telegraph profession, young as it is, is far more conservative than is good for the advance of telegraphy; and, on the whole, telegraph administrations and staffs have by no means that professional education which is required to conduct practical experiments with a clear understanding, and thence deduce rational conclusions. Thus prejudice was created, which was increased from year to year by authors of school literature writing most discouragingly of the subject.

Thirdly, unfortunately, during all that time no physicist found it worth *his while to investigate* the duplex methods with a view to ascertain

quantitatively what can be expected of them, and how they actually compare, with respect to safety, with single working.

Fourthly, duplex working itself could not progress, because it was neither tried nor investigated, and hence no suggestions for overcoming the difficulty of charge and discharge were called for.

Great honour must therefore be given to Mr. Stearns, who brought up the subject again so prominently, and who by his zeal succeeded in introducing it on a large scale, and so elevated the ingenious methods from the questionable position of "interesting scientific experiments."

I think far less of his idea of introducing condensers or Ruhmkorff's coils to balance the charge and discharge of lines, than of his having taken the neglected child up again, against the prejudice of his own profession, and shown that it could have a healthy existence even in the backwoods of America. I trust that these remarks will not be considered irrelevant in the present investigation, since they tend to show how real progress in one of the youngest branches of applied science may be retarded for a considerable period by nothing but prejudice of the profession themselves, for whom progress should be the first essential; and administrations will see how much the advance of telegraphy will always depend on their recognising and encouraging by experiment inventions that are theoretically sound and tend in the right direction.

General Considerations.

Before entering on the solution of the problem for any particular duplex method, it will be advisable once for all to state definitely the nature of the general question before us. This will not only save time, but the subsequent special solutions can then also be made under a general guide, and thus, being well linked together, the whole investigation will become far more lucid and concise than it otherwise would be.

While in ordinary (single) telegraphy the signals are always produced in the same way, *i. e.* by the signalling current arriving through the line from the distant station, the signals in duplex telegraphy may be produced in either of two ways, essentially different from each other, namely, if the times of sending from the two stations fall together, *i. e.* no current, or double current, or any difference of currents, is in the line, *the signals*, so long as this state of the line exists, are produced wholly *or partly* by the battery of the receiving station. Signals produced in

this way we shall call "duplex signals," and these signals alone indicate the essential difference between duplex and ordinary telegraphy.

If, however, the moments of sending from the two stations do not fall together, the signals are then produced as in ordinary telegraphy, and may be appropriately designated "single signals."

It will be clear, then, that when the two stations are at work at the same time, "duplex signals" and "single signals" must necessarily follow each other in accidental succession. Nay, one and the same signal produced in either station may be partly a "duplex" and partly a "single" signal.

To secure, therefore, regularity of working, the signals produced in either way should be invariably of equal strength.

Further, as in duplex telegraphy the receiving instruments must be always permanently connected up with the line, it is one of the first requirements that the out-going or sent current from any station should in itself have no effect whatever on the receiving instrument of *that* station, in order that the instrument may be entirely free to receive signals from the distant station. Thus we have invariably two conditions to fulfil in duplex working, independent of the particular method adopted, namely :—

1. *The receiving instrument of each station should not be affected by its own sending.*
2. *The duplex signals and single signals must be of equal strength.*

If these two conditions, which are necessary and sufficient, could be always fulfilled, duplex telegraphy would be entirely on a par with single telegraphy, for the sending would not only not interfere with the receiving—the more important condition of the two—but the received signals would also be constant in strength, and therefore frequent adjustment of the receiving instrument would be no more required than in single telegraphy.

Theoretically of course every duplex method hitherto suggested fulfils these two conditions, otherwise the method would have to be rejected *a priori* and could not find any place in this paper.

Practically, however, the different methods may behave very differently with respect to the fulfilment of these two conditions, nay, even one and the same method is sure to give quite different results in this respect by only altering the magnitude of the resistances of which the arrangement consists. For in practice, variations, especially in virtue of the line having

by no means a constant electrical condition, are necessarily going on. These unavoidable variations it is clear may cause very different quantitative disturbances of the two conditions (1) and (2) either if we compare different methods or the same method under different resistance arrangements.

To make the foregoing clear, we will designate :—

By p the force which acts on the receiving instrument on account of not being able to fulfil the first condition absolutely ;

By P the force which acts on the same instrument, when the distant station is sending *alone, i. e.* " single signals ;"

And by Q the force which acts on the same instrument, when both stations are sending *simultaneously, i.e.* " duplex signals."

Then the first condition (1) is expressed by :—

$$p = 0 \quad \ldots\ldots \quad \text{(I)},$$

and the second (2) by

$$\mathrm{P} - \mathrm{Q} = 0 \quad \ldots\ldots \quad \text{(II)}.$$

Further, if p cannot be always kept rigidly equal to zero (on account of unavoidable variations in the system) we should at least have :—

$$\frac{p}{\mathrm{P}} = \mathrm{D} \text{ as small as possible} \quad \ldots\ldots \quad \text{(III)},$$

and, if P cannot be always kept rigidly equal to Q, we should at least have—

$$\mathrm{P} - \mathrm{Q} = \mathrm{S} \text{ as small as possible} \quad \ldots\ldots \quad \text{(IV)};$$

p, P, and Q being functions of the resistances and electro-motive forces of the system, which are known as soon as the particular duplex method has been selected.

The general problem which is to be solved for duplex telegraphy may now be clearly stated as follows :—

D *and* S *are two known functions which must be rigidly equal to zero when no variation in the system occurs ; and which for any given variation in the system must be as small as possible, and approximate rapidly towards zero as the variation in the system becomes smaller and smaller.*

Thus the solution of the problem for any given duplex method will always be a question of the Minima and Maxima Calculus.

Having then ascertained the best arrangement for each duplex method, the methods can be compared *inter se*, and that method will be the best and should be selected for use which for any given variation in the system gives the least absolute magnitude to the functions D and S.

If we suppose, however, that the particular duplex method is not given, the problem to be solved becomes more general, but would still be entirely within the limits of the Variation Calculus, furnishing no doubt a very interesting and important application of that most powerful mathematical instrument. The general solution would at once determine the best method possible, after which special solutions would give the best arrangement for that best method.

It is, however, not my intention to endeavour to solve here the duplex problem in this most general form. To be able to indicate so general and desirable a solution is by no means identical with being able to effect it. The task before me is far more simple, since, as already pointed out, I shall investigate each duplex method separately to determine its best quantitative arrangement, and ultimately compare the different methods to ascertain their relative values.

To do this, the question may be attacked in two different ways, depending on the purpose for which the solution is required.

Namely, either the solution is to be made when considering the line as a variable conductor only, but not acting perceptibly as a Leyden jar; or the line is to be considered as constant in conduction and insulation, but acting as a Leyden jar of large capacity. In the first case the solution would be directly applicable to short overland lines (not over 200 miles in length), and in the second case to submarine cables, which, if good, may always be considered sensibly constant in conduction and insulation.

Further, as a long overland line acts both as a variable conductor and as a Leyden jar of sufficiently large capacity, it would then be necessary to give a solution with respect to both these effects. To obtain, however, the same result without rendering the problem too intricate, it will be best to separate the two questions from the beginning, and afterwards combine their solutions judiciously for application to the case of overland lines.

1*st* Problem. *What is the best arrangement of any given duplex method when the line is regarded as a variable conductor, but not as acting perceptibly as a Leyden jar?*

2*nd* Problem. *What is the best arrangement of any given duplex method when the line is regarded as a Leyden jar of large capacity, but not as a variable conductor.*

The second problem may be expressed more clearly as follows:—

2nd Problem. What must be the distribution of condensers along a

given resistance in order that the two essential conditions (I *and* II) *may be least disturbed for a speed of signalling variable between two fixed limits ?* *

It is clear that the nature of these two problems is very different, because in the first we have to deal with forces constant with respect to time, while in the second the forces acting are functions of time, *i. e.* of

* A telegraph line always acts as a condenser with capacity and conduction resistance in each point of its entire length, while an artificial condenser, such as a Leyden jar, which we are enabled to produce sufficiently cheaply, has only capacity but no perceptible conduction resistance in each point. This is in fact the essential difference between a line and a condenser, and, therefore, in order to render their charges and discharges under the same circumstances as nearly as possible equal, as is required for duplex working, it will be necessary to find the law according to which to distribute a certain given system of condensers along a given resistance.

This law will clearly be a function of the signalling speed within its limits of variation. For instance, say the signalling speed is constant, or its range zero, then clearly one condenser connected to any point of the given resistance would suffice; only the magnitude of the capacity of this *one* condenser would be determined by its position with respect to the resistance, and in addition to this would of course be fixed by the signalling speed and the known capacity of the line.

Further, say the speed of signalling is variable between 0 and ∞, or its range is infinite, then clearly only an infinite number of small condensers, distributed along the given resistance in the very same manner as the capacity is distributed along the line, would strictly answer the purpose; in fact, the condenser required in this imaginary case would be nothing more or less than a second telegraph line, identical with the one used for signalling. In practice, however, the speed of signalling varies only between narrow limits, and therefore the number of condensers required to produce as nearly as possible the action of the line with respect to charge and discharge will become few, especially if the best system of distribution has been determined. Until this law is known we can do nothing but find it approximately by experiment, however tedious it may be to do so.

It has also been proposed to use Ruhmkorff's coils for balancing the effect of charge and discharge. This method, however, I believe must be always much inferior to the one of using condensers, inasmuch as the strength of a voltaic induction current scarcely depends on the speed of signalling, while the charge and discharge of a line, it is well known, are not at all an inconsiderable function of the signalling speed.

Therefore, if the strength of the induction current had been adjusted to balance the charge and discharge of the line for a certain signalling speed, the balance would be considerably and at once disturbed if the speed varied even slightly; and, since so long as hand-signalling is used a certain variation in the speed of signalling will always exist, this method will prove a failure, or at all events will render fresh adjustments more frequently necessary than when condensers are used.

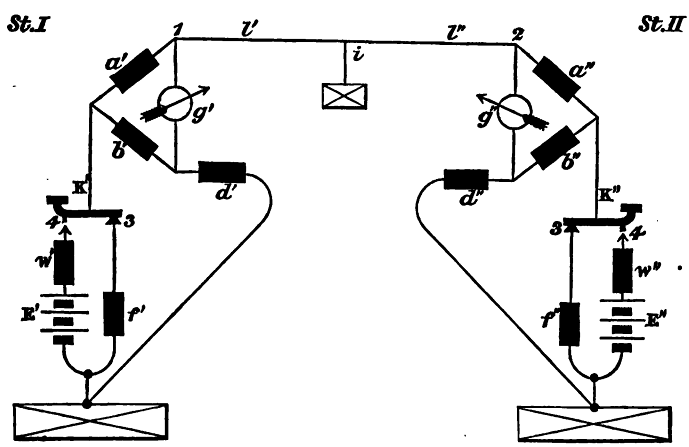
the signalling speed. (The forces in this case are proportioned to the *true currents*). The latter problem being far the more intricate, and for my special purpose only of secondary importance, I shall begin with the solution of the first.

SOLUTION OF THE 1ST PROBLEM FOR ANY GIVEN DUPLEX METHOD.

What is the best arrangement of any given duplex method when the line is regarded as a variable conductor, but not as acting perceptibly as a Leyden jar?

I. The Bridge Method.*

This arrangement for duplex working is based on the well-known method of comparing electrical resistances "Wheatstone's Bridge," and figure 1 gives the general diagram, when this method is applied for duplex working.



* Dr. Werner Siemens mentions this method in Pogg. Ann. vol. xcviii. p. 122, 1856. Mr. O. Heaviside, Phil. Mag. vol. xlv. 1873, states that Mr. Eden of Edinburgh claims to have suggested this method at about the same time as Mr. Stearns of Boston, U.S. America, took out a patent for it.

Explanation of Diagram.

E, electromotive force of the signalling battery.

β, internal resistance of the signalling battery.

k, telegraph-key of peculiar construction, to be described hereafter.

g, the receiving instrument connected up in that branch of the bridge which when measuring resistances would contain the galvanometer. The letter g represents also the resistance of the receiving instrument.*

a, b, and d are the branches of the bridge.

f, the resistance between the rest-contact of the key and earth.

w, an additional resistance to be inserted in the battery branch for reasons to be given further on.

i, the resistance of the resultant fault ("real absolute insulation" of the line) acting at a distance l' from station I and at a distance l'' from station II (both l' and l'' expressed in resistance, so that $l' + l'' = l$ equal the "real conductor resistances" of the line).

Further :

L', the "measured conductor" † resistance of the line when measured from station I,

$$\therefore \mathrm{L}' = l' + \frac{i\,l''}{i + l''}$$

L'', the "measured conductor" * resistance of the line when measured from station II,

$$\therefore \mathrm{L}'' = l'' + \frac{i\,l'}{i + l'}$$

ρ', the complex resistance of the duplex arrangement in station I, *i. e.* the resistance between point 1 and earth.

ρ'', the complex resistance of the duplex arrangement in station II, *i. e.* the resistance between point 2 and earth.

To be quite general we must suppose that the telegraph line, which connects the two stations I and II, has a different resistance when measured from station I than when measured from station II, and that therefore the best resistance arrangement of station I must be also different from that of station II with respect to magnitude of resistances.

* Siemens's polarised relays are well adapted for this purpose on account of their great sensitiveness and wide range ; d'Arlincourt's relays would also answer well.

† Generally these measured values L' and L'' will be different from each other, especially for long overland lines. They can become equal only under two conditions, either if the resistance of the resultant fault (i) is so great that the total conductor resistance of the line ($l' + l'' = l$) can be neglected against it, or for any magnitude of i, if the latter has a position in the middle of the conductor, *i. e.* when $l' = l'' = \frac{l}{2}.$

The resistances which are similarly situated in both the stations will be designated by the same letters, and, to indicate the station to which they belong, each letter will have *one* accent in station I and *two* accents in station II.

Further, if a relation between the resistances of one station has to hold good between those of the other station also, the letters will be used without any accents.

The great practical advantage of the bridge method, it will be clear at once, is that any kind of receiving instrument which has been used for single working may also be employed for duplex telegraphy. This fact must always be of great consideration for any administration that contemplates the general introduction of duplex telegraphy.

General expressions for the two functions "D" *and* "S":—

To obtain the functions D and S we have first to develope the general expressions for the forces p, P, and Q, say for station I.

By p' we understand the force which acts on the receiving instrument g' of station I when that station is sending alone. (Station II at rest.)

p'' in our particular case is therefore proportional to the current which passes through the galvanometer in a Wheatstone's Bridge when balance is not rigidly established, thus

$$p' \propto E' \frac{\Delta'}{N'},$$

where $$\Delta' = a'\,d' - b'\,(L' + \rho'') = a'\,d' - b'\,c'$$

and

$$N' = g'(b' + d')(a' + c') + f'\{g'(a' + b' + c' + d') + (c' + d')(a' + b')\} + a'\,c'(b' + d') + b'\,d'\,(a' + c').$$

Further, by P$'$ is understood the force which acts on the receiving instrument in station I, when station II is sending alone: *Single Signals.*

This force in our particular case is proportional to the current which passes through the receiving instrument of station I when station II is sending alone, and we have consequently

$$P' \propto C''\,\mu'\,\psi'$$

where C$''$ is the current which enters the line at point 2, when station II alone is sending; C$''$ μ' the part of this current C$''$ which arrives actually at point 1 (on account of leakage between points 2 and 1, a part of C$''$ is lost), and C$''$ μ' ψ' that part of the current C$''$ μ' which ultimately produces the signal (*single signal*) in station I. The current C$''$ μ' arriving at point 1 branches off in two, one part goes through a' and the other *through g' to earth.*

Further $$C'' = E'' \frac{m''}{N''}$$

$$\therefore P' \propto E'' \frac{m''}{N''} \mu' \psi'$$

where $$m'' = g'' (b'' + d'') + d'' (a'' + b'')$$

$$\mu = \frac{i}{i + l' + \rho'}$$

$$\psi' = \frac{f'(a' + b') + a' (b' + d')}{(f' + d') (a' + b' + g') + b' (a' + g')}$$

and N'' an expression identical in form with N'.

Further, by Q' we understand the force which acts on the receiving instrument of station I, when both stations are sending simultaneously: *Duplex Signals.*

This force is again proportional to the current which under these circumstances passes through the receiving instrument g' of station I.

This current can be expressed by

$$E' \frac{b'}{n'} - \sigma' \phi' ;$$

and therefore,

$$Q' \propto E' \frac{b'}{n'} - \sigma' \phi',$$

σ' being the current actually in the line at point 1 when both stations are sending simultaneously; and this current, being the algebraical sum of two currents, may be either $+$, 0, or $-$. We will suppose that σ' contains the sign itself.

Further we have

$$\sigma' = \frac{E' m'}{N'} - \frac{E'' m''}{N''} \mu'$$

$$n' = (b' + d' + f') (a' + g') + b' (f' + d')$$

and ϕ' is a function which becomes identical which ψ' if we put

$$w' + \beta' = f'.$$

Therefore the two functions D and S are for the bridge method (station I) most generally expressed as follows:—

$$D' = \frac{E'}{E''} \frac{N''}{N'} \cdot \frac{1}{\mu'} \frac{\Delta'}{m'' \psi'} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(III}')$$

$$\text{and } S' = E'' \frac{m''}{N''} \mu' \psi' - \frac{E' b'}{n'} + \sigma' \phi' \quad \ldots\ldots\ldots\ldots \quad \text{(IV}')$$

and similar expressions will be obtained for station II, namely

$$D'' = \frac{E''}{E'}\frac{N'}{N''} \cdot \frac{1}{\mu''}\frac{\Delta''}{m'\,\psi''} \quad \ldots \ldots \ldots \ldots \ldots \ldots \quad (III'')$$

and $$S'' = E'\frac{m'}{N'}\mu''\,\psi'' - \frac{E''\,b''}{n''} + \sigma''\,\phi'' \quad \ldots \ldots \ldots \ldots \quad (IV'')$$

Rigid fulfilment of the first condition, i. e. $D = 0$:—

For station I we have $D' = 0$,

which equation can only be satisfied by $\Delta' = 0$

since the other factor of D' cannot become zero for quantities larger than 0 or smaller than ∞. Then substituting for Δ' its value we have

$$a'\,d' - b'\,(L' + \rho'') = 0 \quad \ldots \ldots \ldots \ldots \quad (V')$$

or balance in station I, when that station is sending and station II is at rest, must be rigidly established.

Therefore, if balance in station I is disturbed, say by L' varying or by any other cause* external to L', we must have means of conveniently re-establishing balance without delay. This of course could always be done by altering either all the branches, a', b', and d', or any two of them, or only one of them; but it is clear that, so long as the variation of L' which disturbs the balance does not exceed certain limits, balance may be regained by altering only *one* of the three branches available, and as this will also be more convenient in practice than altering two of the branches, or all three simultaneously, we shall make the supposition that—

"*Balance is re-established by an appropriate re-adjustment of one of the three available branches.*"†

The question therefore is, which of the three branches, a, b, or d, is the best adapted for the purpose?

To decide this we must remember that for station II, in accordance with the first condition ($D = 0$), a similar equation has to be fulfilled, namely:—

$$a''\,d'' - b''\,(L'' + \rho') = 0 \quad \ldots \ldots \ldots \ldots \quad (V'')$$

* Causes of disturbance to balance external to L' are inappreciable in practice and therefore may be neglected from the beginning.

† Finally, when the best resistance arrangement has been found, the resistance of the different branches will be expressed in terms of L, and therefore to keep the best arrangement when L varies between any two given limits will involve necessarily a simultaneous alteration of the resistance of all the branches.

If, however, the variation of L is small in comparison with L itself, an alteration of *one* branch for the purpose of re-establishing balance is justified, and would be *absolutely correct if* the variation of L were infinitesimal.

Now ρ', the complex resistance of the arrangement in station I, is a function of all the resistances in station I, and similarly ρ'', the complex resistance of the arrangement in station II, is a function of all the resistances in station II. Therefore, generally, if in order to obtain balance, say in station I, any of the three branches a', b', d' were adjusted, ρ' would alter in consequence of this re-adjustment, and thereby the balance in station II (equation V″) would be disturbed, and *vice versâ*. In other words, the re-adjusting in one station would interfere with the balance in the other station, and therefore rigid balance could be only attained after a series of successive adjustments in both the stations, and then only from a theoretical point of view, approximately, introducing practical difficulties almost insurmountable.

However, examining the positions of the three branches, it will be seen at once that b acts as the galvanometer branch of a bridge for any current arriving through the line. Thus, if we were to fulfil the condition

$$a\,d - f g = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(VI)}$$

for both stations, the value of ρ would become at once independent of b,* and consequently any adjustment of b' to re-establish balance in station I would not affect in the slightest degree the balance in station II, and *vice versâ*.

Thus, pre-supposing the fulfilment of this condition (equation VI) for both the stations, the branch b would evidently be the best suited for adjustment.† Under these circumstances it would then be clear that balance in either station can be obtained by a *single* adjustment of b, and therefore we may call equation VI "*the immediate balance condition*," and the fulfilment of this condition, being of the greatest practical importance to ensure the success of duplex working, we are justified, nay even compelled, to use this relation (equation VI) as the basis for all subsequent investigations.

We will therefore suppose henceforth that

$$a\,d - f g = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(VI)}$$

is rigidly fulfilled for both the stations.

But, as the value of f depends on the position of the key, which during

* $$\rho = \frac{(g+d)(a+f)}{a+d+f+g} - \frac{(a\,d - f g)^2}{\mathrm{F}(b)}.$$

Therefore, if $a\,d - f g$ is very near zero, ρ becomes most rapidly independent of b.

† Further, it must be remarked that, even if the condition $a\,d - f\,g = 0$ be not *rigidly* fulfilled, still by adjusting in the branch b we have "*accelerated*" balance, whereas by adjusting in a or d we should on the contrary have "*retarded*" balance.

signalling moves from contact 3 to contact 4 and back, the rigid fulfilment of equation (VI) necessitates at once that

$$w + \beta = f \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(VII)}$$

not only for both the contacts 3 and 4, but also for all the intermediate positions of the key. Thus, supposing that $w + \beta = f$, *i. e.* the resistance from contact 4 through battery to earth equal to the resistance from contact 3 to earth, a key constructed in such a way that contact 4 is not broken before contact 3 is made, and that contact 3 is not broken before contact 4 is made, would fulfil the required condition entirely. Keys of this kind can be easily enough constructed. It is true that in any such key there will be always a moment when the contacts 3 and 4 are simultaneous, and when therefore the resistance to earth is not f, as it ought to be, but only $\frac{f}{2}$. If it is, however, considered that the time during which this error lasts is very small compared with the time it takes to make a signal, its disturbing effect will never be appreciable in practice, *i.e.* ρ will remain sensibly constant during the time the key is moved to produce a signal.

There will be no practical difficulties connected with the fulfilment of equation (VII), and therefore also none with the fulfilment of equation (VI); for β, the internal resistance of the signalling battery, is the only quantity which of itself can alter in time. However, this variation of β for any efficient form of signalling battery being invariably steady and small, it will be always possible to neutralise its action in time by a simple re-adjustment of w.

If Leclanché's cells are used, or well-prepared Minotti's, a weekly adjustment of w should be sufficient. The measuring of β will always be an easy matter.*

Rigid fulfilment of the second condition, i.e. $S = 0$:—

The general expression for S' was

$$S' = \frac{E'' m''}{N''} \mu' \psi' - \frac{E' b'}{n'} + \sigma' \phi' \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(IV}')$$

* My friend Mr. R. S. Brough suggested the following very simple method for keeping

$$w + \beta = f \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; \text{(VII)}$$

Insert a small galvanoscope in the branch b, for which balance is established with respect to the received current, *i.e.*

$$a\,d - f\,g = 0 \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; \text{(VI)}$$

Now note the deflection on the galvanoscope when both stations are sending simultaneously, and *again when* the station for which β is to be measured is sending alone; *then clearly, if these* two deflections are equal, $w + \beta$ must be equal to f. If the two *deflections are not equal* then alter w until they become equal. After the determina*tion is made, the* galvanoscope is short-circuited.

Remembering that by equation (VII)

$$w + \beta' = f'$$

we know that $\psi' = \phi'$, and substituting further for σ' its value, the general expression for S′ becomes—

$$S' = \frac{E'' m''}{N''} \mu' \psi' - \frac{E' b'}{n'} + \left\{ \frac{E' m'}{N'} - \frac{E'' m''}{N''} \mu' \right\} \psi' \ldots (\mathrm{IV}');$$

and this form of S shows at once that it is perfectly immaterial for duplex working by the bridge method whether the same or opposite poles of the two signalling batteries be put to line,* for in both cases equation (IV′) becomes

$$S' = \frac{E' m'}{N'} \psi' - E' \frac{b'}{n'} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (\mathrm{IV}')$$

Further, it will be seen that the right-hand member of equation (IV′) can be transformed† into $E' \frac{\Delta'}{N'}$, which is equal to p', or we have generally

$$S = p,$$

i.e. the difference of forces by which duplex and single signals in the same station are produced is equal in magnitude and sign to the force by which balance in that station is disturbed.

Consequently the rigid fulfilment of the first condition ($D = 0$) will entail the rigid fulfilment of the second condition ($S = 0$), and this it will be clear is only due to the fact that the complex resistance ρ is independent of b, and that the key during signalling does not alter ρ; whence it follows that the perfection of the key in this respect is of the greatest importance. There are, however, no practical difficulties connected with the construction of a key which fulfils condition (VII) perfectly.

By the aid of the relations given in equations (VI) and (VII) we

* In practice however I prefer to put the same, namely, the positive, poles to the line, as then defective insulation will not be felt so much.

† We have—

$$\psi = \frac{k}{n},$$

$$N = \frac{m k - \Delta n}{b},$$

$$\therefore S = \frac{E b \Delta}{m k - \Delta n},$$

$$= \frac{E b \Delta}{b N},$$

$$= \frac{E \Delta}{N} = p.$$

have therefore gained the great practical advantage, that duplex telegraphy will be entirely on a par with single telegraphy if the means of attaining rigid balance are sufficiently accurate, convenient, and rapid.

But, even supposing that we are unable to keep that balance rigidly for any length of time (on account of L varying), we can nevertheless bring the regularity of duplex working as near as possible to that of single working by making D and S as small as possible for any given variation of L.

Rapid approximation of the two functions D *and* S *towards zero.*

For station I we had—

$$S' = p' \propto \frac{E'\, m'}{N'}\, \psi' - \frac{E'\, b'}{n'} \quad \dots\dots\dots \text{(IV}');$$

which we may also write—

$$S' = p' \propto \frac{E'\, b'}{n'} \left\{ \frac{1}{1 - \dfrac{\Delta'}{m'\, \psi'}} - 1 \right\} \quad \dots\dots\dots \text{(IV}'),$$

$$\text{since } \frac{m'}{N'} = \frac{b'}{k' - \Delta' \dfrac{n'}{m'}}$$

$$\text{and } \psi' = \frac{k'}{n'}.$$

Further, if we call b' the value of b, which in station I establishes rigid balance for any given values a', d', and L$'$, we have—

$$\Delta' = b'\, \delta\, L',$$

where δ L$'$ is the variation L$'$ which throws the balance out, and which variation may be either positive, zero, or negative (δ L$'$ shall contain the sign in itself).

Further, substituting

$$\frac{m'\, \psi'}{b'} = y',$$

$$\text{and } \frac{E'\, b'}{n'} = G',$$

the expression for S$'$ may be written as follows :—

$$S' = p' \propto G \left\{ \overbrace{\frac{1}{1 - \dfrac{\delta\, L'}{y'}} - 1}^{F'} \right\} = G'\, F',$$

which is the best form of S$'$ for our purpose.

The function S' consists of two factors, namely, of G', which, at or near balance, is proportional to the current by which duplex and single signals in station I are produced, and of F', which at balance $\doteq O$.

Therefore, to make S' as small as possible when balance is disturbed, we can only do so by making F' as small as possible, which is evidently the case, for $y' = \frac{m' \psi'}{b'}$ a maximum. Further,

$$D' = \frac{p'}{P'} = \frac{S'}{P'},$$

$$S' = G' F',$$

and since at or near balance

$$P' \propto G',$$

it follows that

$$D = F',$$

i. e. the first condition is also fulfilled by

$$y' = \frac{m' \psi'}{b'} \text{ a maximum.}$$

Our problem for station I would therefore be most generally solved if we make the function y' a maximum, remembering that the variables contained in y' have to fulfil two condition equations, namely the *immediate balance* (equation VI) and *the balance* (equation V).

Substituting for m' its value, and remembering that

$$\psi' = \frac{a'}{a' + g'}$$

on account of the *immediate balance* condition (equation VI), we get

$$y' = \frac{a' (g' + d')}{a' + g'} + \frac{a' d'}{b'};$$

but

$$\frac{a' (g' + d')}{a' + g'} = \rho'$$

the complex resistance of station I (the expression for ρ has become thus simple on account of the immediate balance condition VI).

Further

$$\frac{a' d'}{b'} = L' + \rho''$$

(on account of balance in station I, being established, equation V).

Thus we have

$$y' = \rho' + \rho'' + L'$$

for station I.

And similarly

$$y'' = \rho' + \rho'' L''$$

for station II.

Therefore the rapid approximation of both the functions D *and* S *towards zero in both stations is obtained if we make the complex resistances* ρ' *and* ρ'' *maxima.*

Now the form of ρ shows at once that it has a maximum for

$$(a + f) = (g + d),$$

which, in consequence of equation (VI), gives at last

$$a = g = d = f \quad \ldots\ldots\ldots\ldots \quad \text{(VIII.)}$$

From the development of this result it will be clear that the relation expressed by equation (VIII) must hold for either station independent of L.

All that now remains is to determine b, and further to fix the absolute magnitude of any one of the branches. Before doing this it is however necessary to inquire what the other factor of S, namely G, becomes in consequence of fulfilling the regularity condition as expressed by equation (VIII.)

The current which passes through the receiving instrument to produce "single" as well as "duplex" signals is at balance expressed by

$$G = E. \frac{a\,g}{(a + g)\left\{L\,(a + g) + 2\,a\,(g + d)\right\}} \times \text{const.}$$

which expression has a maximum for either a or g.

The maximum of G with respect to a, it will be seen, contradicts the regularity condition, since $a = g = d$ could only satisfy

$$\frac{d\,G}{d\,a} = 0$$

if d were negative, a physical impossibility.

However, the maximum of G with respect to g gives

$$\frac{d\,G}{d\,g} = L\,(a^2 - g^2) + 2\,a\,g\,(d - g) = 0$$

which is satisfied by

$$a = g = d$$

This is a fortunate coincidence and speaks well for the bridge method.

Now, substituting for a and d their value g in the expression for the current G, we get

$$G = \frac{E}{4}\,\frac{1}{L + 2\,g} \times \text{const.}$$

and this expression multiplied by $\sqrt{g}$ gives the magnetic effect of the receiving instrument, namely:

$$M = \frac{E}{4} \frac{\sqrt{g}}{L + 2g} \times \text{const.}$$

which has an absolute maximum with respect to g for

$$g = \frac{L}{2}.$$

Further substituting in the balance equation (V)

$$a = d = g = \frac{L}{2}$$

we get $$b = \frac{L}{6} \quad \ldots\ldots\ldots\ldots\ldots \quad \text{(IX)}$$

We have therefore the following two equations by which the problem is generally solved

$$a = g = d = f = \frac{L}{2} \quad \ldots\ldots\ldots \quad \text{(VIII)}$$

$$b = \frac{a}{3} = \frac{L}{6} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(IX)}$$

by L being understood the measured conductor resistance of the line from that station for which the best resistance arrangement is to be calculated.

General Results.

1. The branches of the bridge with the exception of the one lying opposite the line must be equal to each other and severally equal to half the measured conductor resistance of the line.

2. The branch lying opposite the line should be equal to the sixth part of the measured conductor resistance of the line, and in this, the smallest of all the branches, re-adjustment of balance should be made only.

Nos. 1 and 2 necessitate the alteration of all the branches if L, the measured conductor resistance, alters within wide limits. A determination of L will therefore be required from time to time.

From the development of these general results it will be evident that they fulfil the following conditions:—

I. The irregularity of signals in the one station is entirely independent of the irregularity of signals in the other station.

II. The irregularity of signals in each station is due only to balance not being rigidly established.

III. If balance in either station is disturbed, a single adjustment in the branch b *will re-establish that balance.*

IV. Any disturbance of balance will have the least possible effect on the received signals.

V. Maximum current at balance.

VI. Maximum magnetic effect of the maximum current on the receiving instrument.

Journal of the Asiatic Society of Bengal.

(To be continued.)

ITALIAN TELEGRAPHS IN 1873.

From the statistical report on the working of the telegraphs in Italy during the year 1873, which has only recently been published, we learn that the total length of these lines throughout the kingdom was upwards of 22,000 kilometres, as compared with 8,000 kilometres in 1861, whilst the length of wires during the same period has increased from 13,000 kilometres to 70,000 kilometres; the number of telegraphic offices, which in 1861 was only 225, with about 400 instruments, had increased in 1873 to 1,625, with 2,800 instruments. In 1861 the number of telegrams sent on Government service was 180,000, and in 1873 no fewer than 300,000 were sent. The number of private despatches forwarded have increased on a still larger scale, for, whilst in 1861 their number did not exceed 600,000, in 1873 they amounted to 5,040,000. The annual receipts of the telegraphs during the same period have increased from 1,200,900f. to 7,500,000f., whilst at the same time the expenditure has not even been doubled, being 3,300,000f. in 1861, and only 5,400,000f. in 1873. During the course of the year 1873 the network of telegraphs was increased by 697 kilometres of lines and 2,348 kilometres of wires, whilst 122 new offices were opened for public service, and seventy-five instruments added to the old offices. The number of telegrams sent showed an increase of 681,246 on those of the previous year. The service also of telegraphic money-orders has also increased considerably, and, whilst their number had augmented by 34,000, their value has been increased by 9,500,000f. The capital invested in the telegraphic admi-

nistration, which in 1872 amounted to 13,600,000f., in the following year was estimated at 14,250,000f., and the net profits for that year amounted to 2,113,746f. 86c., representing an interest on that capital of about 14 per cent. Comparing the length of lines to the area of the country, we find that the average for the whole of Italy is 15 square kilometres per kilometre of line, or 4 square kilometres of area to 1 kilometre of wire. In the Ligurian provinces the average is the highest, being 9 square kilometres per kilometre of line, and 2 square kilometres per kilometre of wire. The lowest average is for the Island of Sardinia, where the average is 25 square kilometres per kilometre of line, and 16 square kilometres of each kilometre of wire. Comparing these figures with those for other European countries, we find that Italy ranks with Germany, and comes after France and Austria, which countries have 1 kilometre of line to 11 square kilometres of territory, or 1 kilometre of wire to 4 square kilometres of area. Belgium and Switzerland are better provided with telegraphs, whilst Spain and Hungary stand considerably lower on the list. The daily average of messages sent per wire is 73 in Hungary, 68 in Italy, 62 in Spain, 47 in Switzerland, and 34 in Belgium. In 1873 the lines and instruments were struck 296 times by lightning, touching 1,028 poles and 140 instruments, and gives an average of one stroke of lightning for each 66 kilometres of line. During the same year, for the maintenance of the lines in Italy, 12,453 poles, 35,076 kilometres of wire, and 72,665 insulators were used. Each telegraphic office serves on an average five communes, or one for each 181 square kilometres of area, or one for every 16,402 inhabitants. The consumption of materials for the batteries in 1873 was 8,078 glass jars, 34,000 kilogrammes of zinc, 24,000 kilogrammes of sulphate of copper, whilst in the offices 24,000 kilogrammes of paper strips for the instruments, and 150,000 kilogrammes of printed forms for messages were used. The greater number of foreign telegrams were sent from Liguria and Lombardy, whilst the provinces which sent the fewest were the Abruzzi, Umbria, and Calabria. With regard to the telegraphic correspondence between Italy and other countries, 35 per cent. is with France, 24 per cent. with Austria, 12 per cent. with Great Britain, 9 to 15 per cent. with Germany and Switzerland, 5 to 10 per cent. with Belgium, Holland, Russia, and Turkey, and about 1 per cent. for Algiers, Tunis, America, Denmark, Egypt, Greece, Portugal, Persia, Norway, Roumania, Spain, &c. The increase in the number of telegrams *in transitu* for 1873 was 26,546.

TELEGRAPHIC PROGRESS.

The Central American Telegraph Company announce that they have received information to the effect, that the line from Para to Cayenne and Demerara, as also the lines from Trinidad to St. Croix and Porto Rico, have been successfully completed, thus giving direct telegraphic communication between Brazil, the West Indies, and North America. These lines will, in accordance with agreements, become the property of the West India and Panama Company.

The new enemy to our submarine cables which has appeared on our sea-coasts, and which has developed a strange taste for gutta-percha, has been found to be the *Limnoria terebrans*, a little worm about one-quarter of an inch long. The *Limnoria terebrans* is not the only enemy to gutta-percha covered wires. Rats, in several instances, have developed a strong taste for this insulating medium. In Bristol, lately, they succeeded in finding their way—owing to the subsidence of the soil—out of an old drain into the flush boxes in the streets, and devouring the gutta-percha coating of the wires, which were thus exposed. Some even ascended the pipes, helping themselves to a mouthful of gutta-percha at different points. It is probable, however, that the shocks which passed through them, on reaching the copper wire, so astonished their weak nerves that the damage they committed was not serious.

Many of the officers of the late Electric and International Telegraph Company, and also members of the Society, will be glad to learn that their old friend and colleague, Mr. H. Shütz Wilson, is continuing to gain laurels in the pleasant fields of literature for which, at the transfer, he gave up the rougher paths of Telegraphy. His "Studies and Romances," published by King & Co., and "Philip Mannington," a novel published by Tinsley, are rapidly running through their first edition, and are much sought after at Mudie's, Smith's, and other libraries. Many will remember Mr. Wilson's early productions, published twenty years ago, in that pleasant and creditable but very scarce publication *Our Magazine*, produced entirely by officers of the Electric and International Telegraph Company.

Mr. Wilson was Secretary to the Society at its establishment and for some time afterwards.

The following is an account of the erection of a new line in the United States :—

"The materials used are—the American patent compound wire and Brooks's insulator. The poles are some of the best that I have seen, but I cannot definitely say what class of timber is employed, as I am not yet *au fait* in the different kinds of wood which are grown here. Specimens of the compound wire I have seen in England : it is mainly composed of steel or homogeneous iron, with an envelope or coating of copper, and over this an external covering of tin. It is supposed to possess a higher conducting power than the ordinary line wires, and consequently need not be so large or heavy as they are. After the erection of the poles, with the insulators fixed to them in the ordinary way, the process of running and stretching the wire is commenced. The wire, which is prepared in coils of a mile in length, is placed on a drum (one coil at a time) ; this is fixed to a small wagon, and, after shackling off, away they start, uncoiling the wire as they go. The entire wiring gang consists of but five or six men—one to attend to the wagon ; two to bind in the wire to the insulators, *i.e.*, one at a pole ; and two or three for stretching the wire, which is done by hand. The horse moves off ; the two men, having strapped the climbing-irons on to their legs, ascend the poles and hook the wire on to the insulator. Speaking of these climbing-irons, they seem to me to be wonderfully good things for telegraph work, and I am surprised that no attempt has ever been made to introduce them into England. They have a spike on the inner side of the instep of each foot, and by means of them a pole is scaled as easily as if a ladder were employed, whilst they dispense with all the trouble and expense of carrying it about. The wire, being hooked into the insulator, is then pulled very tight by the two or three men whose special duty this portion of the work is, and finally bound in—by the men up the poles—with pieces of the ordinary binding wire some four inches in length. Each of these operations occupies almost less time than it takes me to describe them ; and, although it appears next to incredible, I have seen the gang of whom I speak run, stretch, and fasten off as much as 16 miles of wire in a day, not in one solitary instance, but on several occasions. I have never seen a wire stretched tighter with the vice ; in fact, I think they err out here in pulling their wires too tight ; it would never do to pull the ordinary line wires like this : however, if they weather the frost, a fow ohms in the resistance will doubtless be saved ! The lines are worked in closed circuit."

TELEGRAPHIC PROGRESS IN 1874.

Of the progress of telegraphy during the past year much may be said, for many important works have been carried out which have brought the commercial relations of various parts of the world closer together, and have bound so many countries in such near telegraphic alliance that now scarcely a place can be found which is not already in electrical communication with the rest of the world; of such few points which still are without this modern necessity schemes are already on foot to bring them within the "girdle." Whether we look at home or abroad telegraphic progress appears equally satisfactory.

Glancing at the progress made at home, the continued success of the transfer of the telegraph to the State at once attracts our attention. The abandonment of the old Electric Company's head office in Telegraph Street, which had been found far too small, and the simultaneous occupation of the new instrument galleries at the General Post Office, took place at the very commencement of the new year with the greatest success, and from the moment of change everything progressed satisfactorily.

The change was indeed an important one, when it is considered that the whole internal and external communication of London, its pneumatic tube system, and everything, had to be suddenly changed from one centre to another, and yet the transformation was accomplished in but a few minutes. Arrangements had been progressing for a long period to perfect the transfer, the battery room charged with more than 20,000 cells arranged systematically for the London and provincial circuits, the instrument galleries subdivided into sections, the counters provided with instruments and wires from the testing and battery boxes; although the change occupied weeks in its arrangement, the actual accomplishment of it was carried out in less than six minutes. This operation reflected the highest praise on the electrical staff engaged.

The pneumatic system of the Post Office, by the removal of the head station further west, the opening of new "tube" stations, and the duplication of many of the lines, has been largely increased, and consequently increased engine power had to be obtained. The engines and boilers, designed by Eastons and Anderson, have been found to do their work admirably, and the various arrangements connected with the pres-

sure and vacuum pumps, the sending and receiving valves, are considered most complete.

An important change has been effected during the year by the more complete adoption of the "Sounder" instrument. This is a step in the right direction, and the "Sounder" will eventually become the principal instrument in use by the department. Its introduction will be slow and gradual, but unquestionably its use will be found attended with the greatest success.

The Duplex system has been found to answer admirably, and, where business had increased to such an extent as to require extra accommodation, has been at once introduced to the improvement of the working. On short circuits the ordinary Duplex system has been used, but in longer circuits the system known as "Stearns's" has been adopted. At the present time the total mileage of wire working on the Duplex principle is over 12,000 miles, the largest circuit being 450 miles. For news traffic especially, the Wheatstone automatic continues largely to be introduced, and without such an instrument it would seem difficult to carry on so great a traffic.

The general working of the telegraphs throughout the empire has been most satisfactory; the breakdowns due to wind, frost, and snow, have not been more than usually numerous, although great severity of weather has been felt; the precautions taken in preserving the telegraph plant of the country in an efficient state have proved effectual in enabling it to withstand the attacks of weather. By sea the department has not been so successful, for there has been a series of interruptions to the submarine cables. The Channel Islands cables, between Jersey, Guernsey, and Alderney, have been interrupted by the cables parting; communication between Dartmouth and Guernsey has been twice interrupted (remaining so at the present moment); the Irish cables between Abermawr and Wexford, and Holyhead and Dublin, various of the Scotch cables, and many smaller cables have required repairs.

The repairs to the Wexford and Dublin cables revealed a state of affairs which has a serious bearing upon the future of these and many other cables. It was found that certain worms of the "boring" species had made havoc with the hemp, and that others of a smaller kind had not only scored and marked the gutta-percha, but had penetrated directly inward to varying depths, even to the conductor, for one actual fault

was caused by one of these worms, a small insignificant looking object of less than ¼ in. long. Their presence has been known in the south and in various places, but this seems to be their first appearance in the Irish Channel. Wherever an opening appears in the cable the worms enter, the one kind destroying the hemp whilst the other bores directly inward to the conductor. This is an enemy which must deserve especial attention in order to protect its attacks.

The cables round the coast, the property of the Submarine Company, and those belonging to the Post Office leased to them, have been generally in good working order. Several accidents have happened and the necessary repairs effected, whilst some important renewals of shore-ends have taken place.

Railway electric signalling appears greatly on the increase, the block system being as rapidly extended as circumstances will admit, successive accidents giving a spur to its further development. Trials have been made with electrical semaphores which appear to have met with some success. To this we shall look forward as an important application of electricity to railway purposes.

Turning our attention abroad, we find great and extended progress made in almost every quarter of the globe.

A fifth Atlantic cable has been laid for the Anglo-American Telegraph Company, whilst an almost successful attempt was made to lay the Direct United States Cable. Although this cable was so far laid as to warrant the officials stating it was "practically" laid, yet so far as business is concerned it is "practically" useless. The amalgamation of the Anglo-American and French Atlantic Companies enabled them to lay a fourth cable in 1873, and gave them a surplus of 900 miles of cable; this with an additional 1,100 miles to be manufactured, it was determined to make a fifth Atlantic cable to be submerged by the Great Eastern. It may here be remarked that the 1865 cable has been practically abandoned, no attempts having been made towards its recovery. The extra cable required was manufactured by the Telegraph Construction and Maintenance Company, and successfully submerged by their officers in the Great Eastern. The voyage was attended by a succession of gales, but the work was completed in September, the total length of this cable between Valentia and Heart's Content being 1837·045 knots. The Anglo-American Company have purchased a repairing vessel to replace the Robert Lowe, which was lost off Newfoundland.

The Direct United States Cable was manufactured in its entirety by Messrs. Siemens Brothers, who had the contract also for laying the cable; for this they had built expressly the Faraday, a cable-ship of very large dimensions. The route of the cable was to be not very dissimilar to the existing routes. The Faraday and the attendant steamers succeeded in submerging the cables between the American coast and Newfoundland (this end was not landed), after great delay occasioned by fog and general bad weather; the vessels, however, returned to England, and subsequently left for paying out from Ireland. The submergence went on successfully until the 10th of September, when, after paying out 600 miles of cable, it parted in deep water in a heavy gale, the Faraday having to haul back in consequence of a slight fault. After very great delay, the weather being so boisterous, the cable was recovered in very deep water; the splice was made and paying out proceeded satisfactorily until near the Newfoundland coast, when, a fault having passed overboard, the cable was buoyed owing to a gale of wind preventing operations. Since then the ends have been lifted, but owing to fogs and gales nothing has been done to render the cable, which is "practically" laid, "practically" useful. A slight fault, however, remains in the cable.

A large amount of cable-laying has been accomplished during the year, especially by the Telegraph Construction and Maintenance Company, who, in addition to the Atlantic Cable, have laid cables from Jamaica to Porto Rico, 647 knots; from Dominico to Martinique, 37½ knots; Madeira to St. Vincent, 1198 knots; Zante to Otranto, 187 knots; St. Vincent to Pernambuco, 1844½ knots; Kilia to Odessa, 349¼ knots; giving a total, including the Atlantic, of 6101 knots. Of this amount about 2500 knots were manufactured during the the past year.

At the close of 1873 the first section of the Brazilian Submarine Company's cable from Lisbon to Madeira remained incomplete. During the past year the cable was recovered, and communication successfully established between those points. Subsequently the sections between Madeira and St. Vincent and St. Vincent and Pernambuco were finished, so that our communications with Southern America are practically complete. The cables of the Western Brazilian and the Montevidean Companies, also the Central American, complete the communication with the River Plate on the one hand and Demerara on the other, where a cable will connect the West India Islands with the South American coast. Eastward a cable has been laid between Barcelona and Marseilles, opening

up a new route of traffic from Spain, the lines *viâ* France, owing to the Carlist war, being almost perpetually interrupted. The same cause led to the removal of the end of the Direct Spanish cable from Bilbao to Santander.

The Black Sea Company was established during the year, and a cable successfully laid for them from Odessa to Constantinople. Several companies have also appeared, but subsequently, owing to the want of the necessary support, disappeared.

Various interruptions have occurred to the many submarine cables during the year, but none can be considered of great consequence as they were all sucessfully and rapidly repaired. The 1865 Atlantic cable, however, as we have said, remains broken down, no attempt being made to repair it. It is generally considered to be practically abandoned.

Of future submarine extensions we may mention that concessions have been obtained for the extension of communication on the Peruvian and western coast of South America, and also from the Cape of Good Hope to Aden *viâ* Natal and Mauritius.

During the year a Submarine Telegraph Conference was held relative to the charges for messages eastward. A satisfactory arrangement between the companies was arrived at, and the scale of charges fixed. The immense value of the telegraph has had an additional increase in the services rendered by it in the recent transit of Venus. By means of the wire, the longitudes of various points of astronomical observatories fixed specially for this occasion have been definitely determined. In meteorology, also, the telegraph continues to lend its assistance, in daily transmitting to various stations news of the coming weather.

We adverted to the loss of the Robert Lowe cable-repairing steamer, and of the fact of her place being supplied by another vessel. Our notice cannot be concluded without referring to the loss of the La Plata on her voyage to the River Plate, which occurred in the Bay of Biscay so very recently. She carried a quantity of cable to complete a missing link in the South American chain, which strangely was broken by the loss of the Gomos carrying out cable for that object.—*Engineering.*

OBITUARY.

MAJOR-GENERAL MEYDAM.

We regret having to announce the death of Major-General Meydam, Director-General of Telegraphs in the German Empire, and Foreign Member of this Society.

The following notice of his career is extracted from "Le Journal Telegraphique:"

"Theodore Meydam was born on the 13th of September, 1827, at Crassen-on-the-Oder, at which town he commenced his education, being subsequently sent to College at Breslau, which he quitted in 1845, after passing the usual examination.

"On the 24th of October in the same year he began his military career as a volunteer in the 5th Division of the Engineers. In 1847 he was made second lieutenant, and on the 31st 1859, captain of the second class in the Engineer Corps. At this period the promising qualities he exhibited resulted in his being sent to Paris for two years to acquire a thorough knowledge of the French language. On his return he was attached to the staff, and shortly afterwards he was promoted to the rank of major.

"In this capacity he served through the Danish campaign in 1864, and in that against the Austrians in 1866. In the former he was engaged in the Siege of the Lines of Duppel and in the assault of that town, and in the latter he fought at Trautenau, Tobitschau, and in the great battle of Koniggratz.

"In the month of March, 1867, M. Meydam was requested by his Government to master the details of the telegraphic service, and for this purpose he was attached to the Prussian telegraphic system. Six months later he re-joined his ordinary military duties, and on the 1st July, 1868, he was nominated Chief of the Engineer Section of the War Office.

"Meanwhile, Major-General Chauvin, Director-General of Prussian Telegraphs, finding that his official functions were telling on his strength, expressed a desire to be relieved of his office, and M. Meydam was, on the 26th December, 1869, appointed Assistant Director-General.

"On the opening of the Franco-German war, M. Meydam instantly

vacated his civil post to take command of the military telegraphs at the head-quarters of the King of Prussia, and this position he held until the close of the war, when he returned to his former appointment of Assistant Director-General.

"Whilst holding this position, M. Meydam represented the German Government in the Telegraphic Conference held at Berne in September, 1871, and at that which opened in Rome on the 1st December following. At both these meetings M. Meydam took a share in the deliberations worthy of the great nation he represented. Beyond this he was charged specially as a plenipotentiary with different special negociations with neighbouring states—notably with the Austro-Hungarian Empire and the Netherlands in October 1871, with Switzerland in January 1872, and later with Luxembourg.

"In October, 1872, Major-General Chauvin having resigned the Director-Generalship, he was naturally succeeded by M. Meydam, who had for some years taken a very active and full share of the management of the system.

"M. Meydam took this appointment without prejudice to his military rank. Two years later, in July 1874, he was promoted to a generalship of brigade, and on the 27th of October in the same year he received the rank of major-general.

"The services of this distinguished man, both military and civil, had earned numerous decorations and orders, both German and foreign, in addition to several war medals. Amongst these we may particularise the latest, which reached him, as it were, on his death-bed. We refer to the order of the 'Red Eagle' of the third class, which was conferred on him by the Emperor on the 17th of January last, the anniversary of the Imperial coronation.

"A week after this mark of esteem the nervous fever, from which M. Meydam had been suffering for six week, terminated fatally, and thereby the country, as well as the Telegraphic fraternity at large, were deprived of the eminent services which might have been expected from one whose administrative abilities and lofty ideas formed a remarkable feature in his character.

"During the short duration of his administration, M. Meydam acquired and retained the attachment and esteem, not only of his subordinates, but also of the heads of those foreign administrations with which he was constantly in correspondence and in personal communication."

INDEX.